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"OBSERVERS" AND *META-OBSERVERS*

LEO L. GLADIN

Michigan State University[1]

The human has a godlike perspective on the universe, and is thereby enabled to envision both its initial and final specifications. Unlike the infrahuman, whose "universe" is organized around itself as the center of all things and "exists" only as long as the organizer survives, the human has discovered an externally existent universe which began its operations long before his arrival upon the scene and which, in all probability, will continue in its restless flux long after his departure. The mythologies of the several human communities indicate that man was unable to simply accept his discovery, but felt a need to locate himself with respect to the universal flux; in keeping with his infrahuman heritage, he assigned to *mankind* a position of central importance in the scheme of things. Thus, although he recognized the existence of a universe *external* to himself, he did not at the same time recognize the Existent as *independent* of himself. Having intentions and purposes himself, he ascribed intention and purpose to his *alpha* and *omega;* being possessed of malice and charity (though not in equal portions) he ascribed such affects to the capricious forces beyond his ken; moved by hope and dread, he added a *hereafter* to his *here*—populating the firmament and the nether regions with ex-mortals whose entity status was defined according to the standards of his province.

Once having defined the universe in anthropomorphic terms, the human was disabled by his creation; he dared not attempt a more accurate definition lest he be struck by thunderbolts hurled by his own created demon. In cases where the demon was tardy of action, he could be certain that his fellows would punish him for disturbing their equanimity. As an observer of the world-scene, the ordinary human saw and believed according to the conditions of his cultural training. Whereas the perspective of the infrahuman is *point-dependent,* in that the organism's location in time and place is its ultimate determinant, the perspective of the ordinary human was—and is— *point-related,* in that he shares with the continuity of organisms a mortal corpus located in time and place and in addition participates in the culture-habits of his local community—whose location in time and place further conditions his individual capacity for observation.

It is apparent that the perspective of the scientist is independent of his relatively fixed location as a corporeal point having existence in a specifiable time and place. Indeed, the essence of scientific method

[1] Current address: General Delivery, West Side Station, Colorado Springs, Colorado.

lies in the divorcement of the observer from his sense-limited point-location, with its attendant liabilities of ordinary human and organismic frailty. By reducing his "self" to the status of a knowledgeable instrument, the scientist becomes a *meta-observer*—a perfect homunculus, or "pure ego" (Gladin, 1960b) which abides by the rules of procedure devised by the continuity of scientists who preceded him, each rule arduously teased out of experiences of human frailty. In his *Physics*, Taylor (1941) offers many examples illustrative of observer-malfunction—the conquest of which represents a form of discovery fully as significant for scientific advancement as are those discoveries which the scientist regards as his main pursuit. His mechanical instrumentation is as much designed to counteract human fallibility as it is to evaluate data from the field of reality; his mathematical and logical systems serve in a similar dual capacity. When the observer himself is an imperfect instrument, knowledge is indeed "relative to the knower," as Ramsperger (1942) believes. On the other hand, any purported item of knowledge which has congruence with a specified aspect of the field of reality remains independently congruent, and is verifiable by any meta-observer anywhere, at any time. When the meta-observer reports the results of a bit of research, his findings will be studied with respect by the community of scientists, regardless of their colors, creeds, or language habits. Such as might tend to reject a resultant because it is at variance with the prejudices of their home province are thereby excluded from the scientific community and relegated to that larger *brotherhood-of-man*—whose sibling rivalries are not merely sanctioned but also encouraged within their several political, ethnic, social and religious domains.

THE STATUS OF PSYCHOLOGY AS A SCIENCE

While it may seem platitudinous to state that the psychologist has a peculiar occupational hazard in that he is an organism among organisms, it nevertheless remains true that he is required to observe and evaluate the relationships between organism and organism, and organism and environment and yet serve only as an instrument. Experimenting on the current crop of freshman students, he arouses their curiosity by his presence and causes them to speculate upon his intentions, his taste in neckties, his marital status, etc. It is difficult for him to conceive of complex organismic relationships without employing a terminology which has an intimate subjective familiarity within his own being. On the other hand, the physicist has not the slightest notion as to how an electron feels within itself, and is fairly certain that it does not observe its observer at all. In pursuit of this happy state of affairs, the earlier behaviorism made an attempt at "physicalizing" psychology by arbitrarily limiting its field of study to the peripheral manifestations of organismic activity; however, the safeguarding device degenerated into an end in itself, in that the periphery of the organism was endowed—by an essentially *human* fiat—with a spurious significance. Since mention of central functions was proscribed, their admittedly important aspects

were allocated to the accessible periphery; the brain cage was reduced to the status of a small echo chamber, and even the thought processes were—hopefully—to be described in terms of implicit speech—observable in the minute tremors of lips, tongue and larynx. Thus, the profound reflections of an Einstein, a Newton, were at once equated with the ruminations of backfence gossips.

"Observer"—the Vulnerable Middle Term

Whereas behaviorism introduced a number of dubious concepts and sterile controversies into psychology (which persist into the present time) and not much solid knowledge of organism-environment relationships, it nevertheless made a significant contribution by reducing the human tendency to anthropomorphize, by promoting a conscious scrupulosity in the use of language, and by insisting upon rigorous controls over the observer in both research and analysis of data. In an attempt to eliminate the human vulnerability of the observer—as the essential arbiter between organism and environment—the behavioristic excursion failed notwithstanding because it tried to impose on reality after an arbitrarily human fashion.

It will be the thesis of the present paper to assert that the middle term, "observer," is of paramount importance to the establishment of a science, and to further assert that scientific status is not conferred merely because a field of study elaborates theories, indulges in experimentation, or employs mathematical techniques or other methods which seem to convey a sense of authority.

The evolution of a science takes the form of an acquisition of a fund of knowledge of reality in the face of human resistance and organismic obfuscation. Man's humanity resisted a heliocentric version of the universe as less satisfying than the cumbersome geocentric notion; Boyle pursued a "cold entity" for a time because his senses, which were evolved relative to the organismic condition, persuaded him (as it had others before him) that physical nature ought to furnish a correspondence with sensation (Taylor, 1941). Once a fund of knowledge was established, together with the instrumentation necessary for its demonstration, humbleness in the face of knowledge became mandatory for the scientist. If Galvani, in all humanity, thought to gain anything by his insistence that the electrical effect he discovered was generated within animal tissue rather than as a consequence of the accidental contact of unlike metals, all he actually achieved was a slightly dishonorable mention in the annals of science; if Galileo recanted under coercion, he did so with a wry understanding of the travesty in which he was forced to participate; although the ecclesiastics burned Bruno in a belated attempt to amend Copernicus, they did not, thereby, alter the visage of reality a whit. The scientist takes his perspective from the broad field of reality, knowing that only the real enduringly *is*. That perspective includes a charitable recognition of human frailty—including

his own—which bears on his potential for achieving a meta-observer status. In addition to human resistance, he has been confronted by a relatively recent popular adulation of Science—and particularly the remarkably progressive physical departments. This *popular* appeal may well provide the motivational background for the psychologist's desire to achieve an unquestionable scientific status as soon as possible. The chauvinistic satisfaction which the laity finds in the "more than," "better than" production of the home province also distorts the scientific quest, in that it discourages the free exchange of scientific information. Such provincialization has a corollary effect, too, and particularly in the case of the immature sciences—the tacit acceptance of the "better than" home product may discourage a serious entertainment of ideas evolved in foreign provinces. Thus, American, British, Soviet, German, etc., psychologies may come to differ one from the other, in sharp contrast to the generality of the physical sciences.

Mediate Filters and the Observer

Historically, it can be noted that the sciences have been subjected to a prior screening for compliance with social, religious or political ideologies which dictated the boundary specifications of scientific fields. Such artificial limitations on the pursuit of knowledge constitute *mediate filters* which render the observer an imperfect instrument. The mediate filter may be explicitly defined or may appear in subtle disguise. Thus, out of veneration for physical science, the earlier "militant behaviorism" rigorously banished the so-called mentalistic concepts from psychology. Such terms as "set," "attention," "purpose," etc., were ignored or replaced by ingenious subterfuges which were covertly translatable; when their employment seemed necessary for communication, they were used apologetically (save by brave men like Tolman, who boldly employed such terms as "purpose" and "expectancy" simply because they were designations expressive of demonstrable organismic activities; in so doing he showed that words, even when they have been reified as "bad things" can nevertheless serve in a useful function, as long as the user clearly recognizes how he is employing them and can convey this information to others.)

Mine, all mine. . .

Freud, (*see esp.* 1938) who exposed his private self to public scrutiny in the interest of exposition perhaps more than any other psychologist, expressed a right of ownership over his findings more than once, and even noted his gratitude that he was "not well read" (unusual modesty, in the light of his extensive bibliography) because he might otherwise have discovered that another had preceded him in some of his major findings, and thus jeopardized his status as an originator. Other psychologists, although not directly expressing rights of ownership, delineate ideas and concepts in such a manner as to differentiate them

from notions which have been in psychology's common domain for years, and hence no longer "discoverable." While it requires no unusual astuteness to identify and classify such discoveries for the most part, they nevertheless serve in an obscurantist capacity in that they represent a human resistance toward synthesis which diminishes psychology as a science, since the author wishes to have them identified with himself, rather than with each other. For science, the name of a human cannot be used as an acknowledgement of ownership of items of knowledge; rather, it is used as a shorthand form of reference to source material. Nevertheless, in psychology the process resolves itself more than occasionally into a compliance with the human fraction—often taking the form of reciprocal back-scratching which serves to perpetuate the unscientific tendencies common to the human condition. When notions are separated from persons and identified with each other, they suffer no pain; should the persons suffer anguish, the problem is not one for science to handle (save, perhaps, for such applied sciences as are concerned with psychotherapeutics.)

Method and Madness

Of Sigmund Freud, Boring (1950, p.707) says: "It is not likely that the history of psychology can be written in the next three centuries without mention of Freud's name and still claim to be a general history of psychology." There are probably very few—if any—other psychologists of whom the same could be said. Yet, at the end of an illustrious career he was exiled from his native land and his books were burned because he was a non-Aryan—hence could not possibly be a scientist according to the provincial definition; near the beginning of his career, he became a kind of scientific pariah for quite other reasons. As was the case with James when he announced his theory of emotions—removing the lively affects from the ethereal soul and relocating them in the pedestrian corpus—a human cry of pain arose when Freud questioned the neatly defined rationality of man and his culture and proposed to make of the innocent babe a sexually active monster, according to the interpretation of the pruriently-minded Victorians.

In addition to and (as Freud suggested) in substitution *for* the human outcry, the scientists so-called protested the *method* by which the theoretical notions were obtained as well as the *method* by which they were subject to verification. In certain quarters, Freud continues to be ignored or minimized because the practitioners are dedicated to the scientific method—i.e., the method employed so fruitfully by the physical sciences.

It is inherent in the very nature of good theory that it will open up hitherto-unthought-of prospects; once a key is hit upon, it will unlock door after door of new possibilities, with the theorizer remaining compliant to the ramifications which each successive idea presents. The *person* may feel ill at ease during the process, since it is upsetting the

status quo of his rational organization. Freud mentioned, somewhat apologetically, that he felt as if he were writing a novel while at the same time he was convinced that he was setting forth the specifications of scientific ideas. On the other hand, no equilibrium-upsetting product will issue from pedestrian theory, since the issues are forced by the theorizer in accordance with premises which constitute a reduction of the complexities of behavioral organization. Whereas the mundane theory is amenable to "scientific method" because it was proposed within its limitations, the productive, though perhaps less elegant, theory may well outstrip the contemporary instrumentation and methodology. Within physics, the question, "Can the atom be forced to yield its energy?" was, practically speaking, meaningless until the preposterous-looking and ponderous machinery was designed to "split" an atom. Being the product of intensive cerebral effort on the part of the community of nuclear physicists, the instrumentation issued from the dictates of theory.

The actual *statement* of theory issues forth within the limits of articulation of the theorizer, so that his product ought not be scorned because it has not at the same time endowed him with preternatural powers enabling him to exposit perfectly that which has been but dimly grasped. Maxwell employed hypothetical "rollers" and other mechanical devices in his exposition of electrical theory (Taylor, 1941) yet did not fail to advance knowledge in physical science; his scientific fellows simply were not disposed to regard the mechanical intermediaries as the major foci of attention, to the end of discounting the significant aspects of his proposals.

However, when a human becomes an *authority* in his field, his theoretical errors assume the status of his demonstrable truths, in the minds of his authoritarian dependents. When the human *insists* on his authority, he diminishes science insofar as he establishes a cult—a closed corporation founded on his ideas. Following Adler's defection, Freud (1938, p. 964 f.) exposed him to what remains the least-expensive analysis ever in the history of the psychoanalytic movement—at the same time revealing a sense of proprietorship over his ideas. Freud's essentially human motives (which were probably reinforced by the derision provoked by his ideas and directed at his person) resulted in an orthodoxy which altered its ideas only when *he* altered *his*, so that there yet remains an identifiable group whose observations are mediately filtered through the dicta of the Master. Since error—unlike knowledge—is inviolately unalterable to the cherisher, only his possibly gross misconceptions remain in the exclusive possession of the orthodox group.

When Watson formally inaugurated the physicalistic movement in psychology, he proposed both the reduction of the possible number of investigatory methods and a limitation on the subject matter to be investigated by the psychologist. Within this abbreviated frame of reference, he boldly—if rashly—proceeded to prescribe formulae for the

molding of the human personality. A more sophisticated behaviorism subscribes to the recommended methodology—which approximates that of the physical sciences—in a dedicated manner. In order to avoid preconceptions and mentalistic biases, the methodist concentrates on *observable* behavior in a systematic way, and ignores the body of knowledge available to the psychologist which has been acquired outside the method. Confining his research to the laboratory, he is reduced to the selection of a readily available, fairly amenable organismic subject—a small rodent, or perhaps a pigeon. Being initially restricted in scope by the selection of his small subject, the experimenter proposes problems which—hopefully—he believes are within the organism's handling capacity. Also hopefully, he believes that by the faithful application of method he will achieve results which can be extrapolated to a range of organisms, in the form of basic laws of behavior. He professes, ordinarily, no especial interest in the human, but as a "pure scientist" is interested in the lawful behavior of organisms-at-large. In the physicalistic sense, he is concerned with quantitative prediction, which is his reason-for-being as a scientist qua scientist.

If the methodist no longer pursues the ambitious program proposed by Watson, it may well be that he has been *reduced to* a pursuit of "basic laws" by reason of his dedication to a single method which both taxes his ingenuity and arbitrarily limits his range of study. His precariously established "basic laws" (which are not uniformly agreed upon even among methodists, although they have been empirically derived) seem to belong in comparative psychology and are often specific to particular artificial situations (as of the rat in a maze). Nevertheless, he is ready to extrapolate his findings to the human sphere, even though he has imposed upon himself a condition of ignorance as to the full implications of complexly ramified human behavior. Often, his extrapolations consist of *sentences about* infrahuman behavior which have a syntactical resemblance to sentences about human behavior; the "empirical results" which he thus endows with significance may have only a superficial resemblance to human manifestations whose actual fundaments are different and more complex. The "empirical backbone" with which he furnishes his statements about human behavior may be an artifact of his ambition to "save the phenomenon" created by—and invested in by— the human-as-scientist. All unwittingly, the results obtained by Method may be thus inflated to a status of significance which can persuade the unwary that the methodist "really has something there."

As one method among many, the method used by the physical sciences has a place in psychology; however, its advocacy may very well be motivated by a human desire rather than by realistic necessity—a desire which proclaims that *here* is The Way into the halls of scientific respectability, whose portals are not to be gained by any other route. If it really is *the* way, it ought to be able to produce more significant results than allegedly *very poor* method. Should it turn out that psychology is not really "like" any other science, even the hope of eventual

exact quantification may be illusory; nevertheless, there is no reason to believe that the *logic* which underlies mathematics and verbal syntax alike would be inapplicable. All that would be required is the consistency of formal propositions among themselves and without contradiction to empirically derived propositions, which must also be consistent among themselves.

Theories, Theorists, and Camp Followers

In psychology, when two conflicting theories of learning are confronted with an *experimentum crucis*, the most predictable conclusion is that both will emerge unscathed, if not victorious. It seems that no *experimentum* is sufficiently *crucis*. The intellectual artistry expended in the service of theory—in the sense of ingenious reinterpretation of data, and of theoretical propositions in cases where the data are recalcitrant—strongly suggests that the theory is being treated like a living thing which must be rescued from jeopardy. Indeed, the whole-souled dedication of the psychologist to his chosen theory suggests that a human factor is involved (perhaps a low tolerance for ambiguity?) which bears on his own quest for security amidst the psychological vagaries. Hilgard (1956, p.14) observes that ". . . a premature systematic position is likely to be dogmatic and bigoted"—but adds that the *desire* for system need not "blind the seeker after it to the truths unearthed by those with views unlike his own." It is likely, however, that the human desire for system engenders the subtle bigotry and dogmatism as well as the blindness; from a scientific standpoint, the most premature systematization is innocuous as long as the theorist and his followers are conscious of their own human vulnerability.

It is in the nature of authority that it supplies by fiat that which is not provided by knowledge whenever an issue is in doubt, and—as a corollary—that the power of authority varies inversely with the available amount of knowledge in a given field. Theory, on the other hand, ought to serve the function of extracting knowledge from the field of reality, whatever the human may will or will-not. From a meta-observational stance, it can be seen that theory is no more powerful than the vulnerable instrument, the observer, allows. If, in the absence of knowledge, theory assumes the status of authority, the *function of theory* is violated. Theory then becomes the mediate filter through which the field of reality is viewed; it may determine not only *what* the observer sees but also *how* he sees it, not only what he reads, but how he understands what he reads.

Whereas in physical science a theory is rendered suspect in proportion to the number of *post hoc* patches which hold it together, this does not appear to be the case in psychology. Instead of serving as a device to free the psychologist from human vulnerability, theory often serves him in an opposite capacity: it provides that "aura of significant intensity" which is discoverable in superstition—where presumption is reinforced by positive instances of coincidence—and psychoanalytic method,

which is accused of what may be regarded as the occupational hazard of observers-at-large. Theory then not only fails to function as a device to test the limits of an idea apart from human desire, but obtains support *from the human fraction.*

The Pursuit of the Monad

Theorists seem partial to the pursuit of a monad (or at most, a dyad) which is to serve as the essential basis for the explanation of complex organismic learning processes. The theorist assigns to his monad ("contiguity," "reinforcement," "stimulus-intensity reduction," etc.) the status of a *first principle* which reduces his freedom to vary in the service of the extraction of items of knowledge from the field of reality, and which subjects at least some of the data of research to unusual strain in order to force a satisfactory fit to theory. What the organism *is* of itself as a complex and in relationship to the physical world as a complex becomes reduced by processing to a level of simplicity dictated by the monadic device. Should learning processes *themselves* have a lesser elegance than the monad makes provision for, the theorist will be disabled from discovering such a possibility. Hilgard (1956, p. 12) observes "Perhaps by using the common name 'learning' to cover the acquisition of motor skills, the memorization of a poem, the solving of a geometrical puzzle, and the understanding of a period in history, we are deceiving ourselves by looking for common laws explanatory of processes that have little in common."

As with a host of other matters in psychology, there is no evil latent in the proposing and testing of a monadic conceptual device; it is when the monad becomes the observer's mediate filter that the spirit of the scientific quest is violated. It then tends to be overdefended, and the intellect of its several defendents is mobilized in its service, so that its role becomes overemphasized and hence persuasively significant to what might otherwise be an impartial observer. Monads (and dyads) which might conceivably be synthesized into a larger whole are preserved in an atmosphere of immaculate separation which serves to interfere with the larger task of psychology as a science. As has been the case with more general issues in the realm of theory—such as peripheralism vs. centralism, continuity vs. discontinuity, etc.—controversies which are, at bottom, artificial in nature tend to be generated. The idea is put forth by some that such controversy is good for psychology because it stimulates research; nevertheless, when the human is set for argumentation his research will tend to set apart such matters as a scientist would choose to bring together, until, as Hilgard (1956, p. 438) says, "... the atmosphere of debate is changed to one of search for the conditions under which various experimental results are found."

SUMMARY AND PROSPECTUS

It is the thesis of those who wish to hasten the day when psychology

will have attained indisputable status as a science that the *method* employed by sciences-of-status is the key to accession. Herein it has been asserted that theorizing, qualification and experimental methodology can provide no such magic key of themselves. If, in the practice of his profession, the psychologist can gain an appropriate detachment of his human "self" from his *scientific self* as a knowledgeable instrument, and thus achieve a status of humbleness in the face of the *possibility* of knowledge, he will have arrived at the stage of advancement reached by the physical scientist *after* he was confronted by a body of knowledge of reality which, willy-nilly, prevented him from intruding his "self" as the mediate filter for his observation. While physical science evolved an applied psychology specific to its task *implicitly* during the course of a scientific quest whose primary aim was the physical sphere, psychology is in a far better position to embark upon a similar program *explicitly*, and thus safeguard its scientific status not by a leap to the method which is itself only a product of knowledge of human and organismic frailty, but by an overleap which comes to grips with the underlying problem.

If, as Hilgard (1956, p. 312) says, "Experimental psychology will always be cut off from some of the more important aspects of psychoanalysis if it insists upon methods lying outside the psychoanalytic process itself," it becomes necessary to use the method imposed by the kind of reality under investigation, and not the method of choice. If, as Hilgard observes (1956, p. 446) "The motivations of psychologists have to be taken into account as well as the ripeness of their theories," then the "motivations of psychologists" become as fit a subject for scientific analysis as are their theories, and if, as Bakan (1953, p. 47-48) says ". . . the psychological processes of the experimenter are an integral part of the experimental situation," such processes are also a proper subject for scientific analysis.

Exposure to knowledge which is not under a prior subjection to method or other device can serve to reduce psychological speculation to the merely possible (*as, see* Gladin, 1960 a, b) as well as open it to the entire range of possibility. If psychology is not really similar to physical science, it thus becomes enabled to use methods imposed by the strictly psychological realities; where an idea cannot find confirmation in experimental method, it may be found in sociology, philosophy, neurophysiology, physics, etc., or obtain partial support from some or all of them sufficient to allow its retention on a tentative basis (with a degree of reservation based on its fit with an aspect of reality rather than the bias of an individual or group.)

The meta-observational consciousness of human frailty, which ought to be more readily attainable by the psychologist than by the professional of any other discipline, should prevent such a program from disintegrating into a "Lo here! Lo there!" harum-scarum approach which would discard or otherwise lose data *because* of method as has been the case in some

of the abrupt shifts which have taken place in psychology. Becoming a meta-observer in the fullest sense of the word, the psychologist would attain the status of scientist without any more formal initiation by rite or prescription.

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SOME ASPECTS OF HUMAN OPERANT BEHAVIOR[1]

DONALD H. BULLOCK

Institute of the Pennsylvania Hospital

Commonsense tells us that most human behavior is voluntary. By definition, voluntary behavior is less amenable to prediction and control than is involuntary or nonvoluntary behavior. This poses a problem for a science of behavior. Scientific understanding derives from the prediction and control of natural phenomena. Any genuinely effective science of human behavior must, therefore, be able to cope with—to predict and control—voluntary behavior.

Commonsense also provides a possible solution to this problem of voluntary behavior. Commonsense tells us that, whatever the degree of voluntariness in human behavior, most of us will, most of the time, behave in a manner which reflects our past experiences. The distinction between voluntary and habitual behavior is an example. By habitual behavior we mean voluntary actions which, through repeated experiences ("practice"), have become relatively automatic and thereby less voluntary: i.e., more predictable and controllable.

This commonsense understanding of the role of past experience in affecting voluntary responses has its counterpart in the psychology laboratory. For example, many psychologists are interested in the behavior of animals. Now voluntary behavior is not strictly a human affair. Animals also exhibit considerable behavioral spontaneity. Thus, in order to study animal behavior experimentally, it was necessary for psychologists to deal with voluntary behavior in animal subjects. To do this, psychologists developed concepts and techniques designed to improve our understanding of the ways in which past experience, or learning, influenced or affected, that is, controlled voluntary actions.

For the purpose of experimental investigations, voluntary behavior can be defined, in its simplest sense, in this way: the subject is in a situation in which it is possible for him to make a particular voluntary response, or not to make it, according to his own "free will," "volition,"

[1] This is a modified and expanded version of papers presented originally at Bryn Mawr College and Denison University, 1959, and Lehigh University, 1960. The research was supported by funds from various sources: PHS Small Grant M-1464, grants from the Lilly Research Laboratories and Wyeth Laboratories, funds from the Theano Foundation (Mrs. E. Paul DuPont), and funds of the Pennsylvania Hospital. Manly Y. Brunt, Jr., M.D., Clinical Director, North Building Service, collaborated in those phases of the research concerned with psychiatric patients. Mr. Richard Sorokin, Mrs. W. Robert Moore, Mrs. George Avery, and Miss Jean Strouse assisted in the conduct of the research. James J. Dixon, Ph.D., Chief, Psychological Service, Per-Olof Therman, M.D., Director of Research, and J. Martin Myers, Jr., M.D., Director, North Building Service, provided invaluable assistance. Dr. Bullock was formerly Research Psychologist, Psychological Service. He is currently a USPHS, NIMH Special Research Fellow in tenure at Bryn Mawr College.

or "choice." Thus, we place the animal, such as a rat, in a small box which contains a lever extending out from one wall. "Pressing the lever" is the voluntary response. The rat is "free" to press the lever or not to press it, as he so chooses.

Having arranged a simple experimental situation which appears to provide an acceptable, albeit very simplified operational definition of a voluntary response, we can now proceed to examine the effects of past experience or learning upon this voluntary response. We can deprive the rat of food and arrange conditions so that when he presses the lever a small piece of food will be delivered into a tray. Under these conditions, the rat will develop the "habit" of pressing the lever for food. The frequency with which the trained or conditioned rat "chooses" to make this voluntary leverpressing response will depend upon such factors as whether or not he has been deprived of food, how frequently he has been rewarded with food, etc. A great number and variety of animal experiments have been conducted which show clearly that the animal's *present* voluntary actions reflect his *past* experiences in the situation, and that his behavior can be predicted and controlled on the basis of knowledge about the nature of his past experiences. Since this type of admittedly simple experimental situation seems to have an obvious relation to many aspects of voluntary behavior in the human, the past quarter century has witnessed considerable application of this general approach to the analysis of voluntary behavior in animals. Fish striking a target, pigeons pecking a key, rats pressing a lever, cats pushing a pedal, and monkeys operating a hand-switch have all contributed to our knowledge of the ways in which past experiences increase the amenability of present voluntary behavior to prediction and control.

During recent years, an increasing number of psychologists have been extending this general approach to the analysis of human behavior. One specific method which has become prominent is the so-called *free operant response* situation developed by B. F. Skinner (1938). In this paper, I will discuss the application of free operant response concepts and techniques to human subjects. More specifically, characteristics of the method will be described, after which aspects of its use with human subjects will be illustrated with data obtained in studies with "normal" humans and psychiatric patients.

CHARACTERISTICS OF THE FREE OPERANT RESPONSE METHOD

When applied to animal subjects, the free operant response method is characterized by the following features:

1. The organism is alone in an experimental chamber (except, of course, when the aim is to investigate social interactions). Usually

he is free to move about, though the organism's freedom of locomotion may be restricted: e.g., where it is desired that physiological measurements be obtained concurrently. The experimental chamber is usually light-proof and sound insulated, thereby minimizing extraneous stimulation and allowing for a high degree of standardization of the experimental conditions.

2. The experimental chamber contains one or more devices which the subject is capable of operating (e.g., a lever which he can press, a key which he can peck, a panel which he can push, etc.). Since these devices, or manipulandi, are continuously available to the subject, he is free to respond whenever he "chooses." In this sense, his responses are *free*. Skinner (1938) has termed *operant responses* those skeletal muscle actions which operate upon the organism's environment (i.e., upon the manipulandum). Thus, the concept *free operant response* refers to the fact that the subject is in an experimental environment in which manipulandi are continuously present, thereby allowing him to emit the experimental responses "at will."[2]

3. The manipulandi contain switches which are activated when the subject operates the device. These switch activations define the occurrences of the experimental responses, thus making it possible to record them automatically on counters, polygraphs, cumulative recorders, etc. This automatic recording of responses provides a permanent record which permits leisurely, repeated, and systematic analysis, and in this sense yields a truly *objective* index. The basic dependent variable is that of the distribution of responses in time. Quite often the rate of responding serves as the main dependent variable, but for more refined analyses the actual distribution of responses in time (i.e., the distribution of inter-response times) and other measures may be applied. It is important to note that the rate of responding per se is not in and of itself the only relevant dependent variable, since in many situations the response rate pattern is especially significant. Too frequently those not adequately conversant with free operant response concepts and techniques have incorrectly assumed that the overall rate per se is the only dependent variable, and have based their criticisms upon this incorrect premise.

Since the mechanical design of the manipulandum necessarily influences the properties of the skeletal muscle activities involved in operating it, design presents important technical problems. Criteria

[2] There are experimental situations in which the reinforcement of the experimental operants is followed either by removal of the manipulandum (e.g., the Wisconsin General Test Apparatus) or by removal of the organism from the apparatus (e.g., T-mazes). These I would call "controlled operant response" situations. In distinguishing between "free" and "controlled" operant situations, I am referring to situational-procedural features and do not imply any specific theoretical distinctions.

which have been pointed up as significant are illustrated by this quotation from Ferster & Skinner (1957, p. 17):[3]

> The response which the pigeon makes in pecking . . . a key has the following desirable properties:
>
> 1. It uses an effector system (the beak, the head, and the neck of the bird) which occupies an important place in the normal activity of the organism.
>
> 2. The effector system has relatively small mass and is capable of high rates of activity.
>
> 3. Successive responses to the key resemble each other closely. The topography of examples of this class of responses can be fairly narrowly specified.
>
> 4. Responses can be reliably recorded and reinforced through their action upon the key.

4. The experimental chamber also contains various stimulus sources which serve two main purposes: (a) the experimental presentation of reinforcing stimuli (food, water, electric shocks, etc., which can act as "rewarding" or "punishing" stimuli); and (b) the experimental presentation of discriminative stimuli (lights, sounds, etc., which can act as "cues"). Temporal and other relations among the discriminative stimuli, experimental responses, and reinforcing stimuli are controlled by automatic programming equipment (systems of electrically operated relays, timers, etc.). The programmer establishes experimental *contingencies*—rules which define the conditions under which responses will be reinforced—and these contingencies constitute the basic independent variables.

5. Given these conditions (1-4 above), the analysis of behavior takes the form of determining the functional relations between response rate patterns (as the dependent variables) and the experimental contingencies (as the independent variables). Specific contingencies are introduced and their effects are determined. Subsequently, changes can be introduced and, when appropriate, parametric analyses can be conducted in which the values of specific components are varied. Similarities and differences in the behavioral effects of various contingencies are examined, and inferences are drawn as to the nature of the functions. These inferences lead to subsequent experiments designed to test the validity of the inferences. Thus, there is much more involved in the analysis of behavior than merely correlating experimental contingencies and response rate patterns. The goal is to discover lawful relations, to find order among the functions, and to develop descriptive schemes within which to comprehend the lawfulness in a systematic manner. The number of specific contingencies which might be studied

[3] Herrnstein (1958) has questioned the validity of the first criterion, and has suggested that an operant not normally utilized by the organism, and thus not having a history of extra-experimental reinforcement, might provide for purer experimental analyses.

is very great. No one has seriously defended the position that all possible contingencies should be examined willy-nilly (though some critics of the "Skinnerian" approach appear to believe that this is what is required). The discovery of order, of lawfulness, depends upon scientific activities about which some controversy exists.[4] Generally speaking, certain contingencies are considered to be fundamental in that they serve as the foundation for analyses of presumably more complex, higher-order phenomena. Among the contingencies which can be treated as basic are those of reinforcement, selective reinforcement, and reinforcement schedules. Any free operant response situation includes (a) various stimulus sources which may come to function as "cues" or discriminative stimuli, (b) one or more experimental operants which can occur in the situation, and (c) stimuli which may exert reinforcing effects. The contingencies of reinforcement, selective reinforcement, and reinforcement schedules define basic relations among these three components. These basic contingencies have been used in the analysis of conditioning ("learning"), stimulus discrimination, response differentiation, and a number of other phenomena.[5]

FREE OPERANT BEHAVIOR IN HUMANS

In order to study free operant behavior in human subjects, we need an experimental chamber (e.g., a small room), a manipulandum (e.g., a lever, a knob, etc.), a source of reinforcement (e.g., money, "social approval" stimuli, etc.), sources of discriminative stimuli (e.g., colored lights, etc.), means for recording the subject's responses (e.g., cumulative response recorders, counters, etc.), and means for establishing the experimental contingencies (automatic programming equipment).[6] Given these, we can investigate a wide range of phenomena experimentally. By varying the types of reinforcers, we can study "incentive" aspects of "motivation." By varying the relations between discriminative stimuli and reinforcement, we can study discrimination and related sensory-perceptual phenomena. By using psychiatric patients as subjects, we can study psychopathological influences. By administering drugs to the subjects, we can study psychopharmacologic phenomena. By requiring two subjects to respond in relation to each other, we can study aspects of "social behavior." Though it might seem that the use of simple motor responses would place severe limitations upon the utility of any situation for analyzing human behavior, experience to date supports the observations of Rodnick & Garmezey (1957, p. 169), who have commented that "It is our contention, too, that the use of a lever response

[4] Education for research in psychology. *Amer. Psychologist*, 1959, 14, 167-179.

[5] For more detailed consideration, the reader may consult texts such as Skinner's *The Behavior of Organisms* (1938), *Science and Human Behavior* (1953), and *Cumulative Record* (1959).

[6] A number of investigators have been conducting studies of "verbal conditioning" in which verbal (usually spoken) operants have been reinforced with "social approval" stimuli presented by the experimenter, who serves as the programmer. Krasner (1959) has reviewed the literature.

does permit the experimenter to use a considerable range of experimenal tasks . . . [and] It is of interest to note that it was solely in the one experimental situation in which patients were required to respond *verbally* . . . that our schizophrenic Ss failed to reach levels of performance equivalent to normal *Ss* under *nonstress* conditions."

MULTIPLE-SCHEDULE BEHAVIOR

A reinforcement schedule is a "rule" which defines the relation between occurences of an operant response and its reinforcement. A schedule often used in animal and human studies is continuous reinforcement, in which every "correct" response is reinforced. There is another important circumstance, however, that in which responses produce reinforcement only on occasion, or intermittently. For example, the organism may be required to produce a certain number of responses for each reinforcement (a ratio schedule), or a certain amount of time must elapse before a response will produce reinforcement (an interval schedule). Considerable data from both animal and human studies now exist to demonstrate that specific reinforcement schedules (e.g., fixed ratio, variable ratio, fixed interval, variable interval, etc.) tend to generate characteristic forms of responding, or response rate patterns (e.g., Ferster & Skinner, 1957).

According to Ferster & Skinner (1957, p. 503), "A multiple schedule consists of two or more alternating schedules of reinforcement with a different stimulus present during each." Multiple schedule procedures have certain appeal as tools with which to investigate behavior. They are efficient, making it possible to examine two or more processes in a single organism. Multiple schedule procedures also make it possible to have a subject serve as his own control, since specific components can be included with which to assess changes in motivation, discrimination, and other variables. A multiple schedule does introduce the problem of interactions among the components, but the problem is empirical and one can determine these interactions experimentally as necessary to clarify the interpretation of results.

The multiple schedule procedure which I have used in studies with human subjects consisted of these components: fixed ratio, fixed interval, variable interval, and nonreinforcement (S^{Δ} or extinction). In selecting these components, I had in mind two aims: (a) to expose the subject to components which should bring about a wide range of response rates, and (b) to expose the subject to components for which a rational interpretation of the resulting performances appeared feasible. Typically, fixed ratio schedules generate high response rates, fixed interval schedules generate low rates, variable interval schedules generate intermediate rates, and nonreinforcement or extinction brings about a cessation of responding. Thus, these four components in a multiple schedule could

be expected to produce a range of response rates: high (fixed ratio), intermediate (variable interval), low (fixed interval), and zero (S$^{\Delta}$).

Behavior generated by these schedules can be predicted from the assumptions that a "normal" subject will behave in such a way as to (a) maximize rewards (obtain each reward as soon as possible and obtain as many rewards as possible), and (b) minimize effort (obtain each reward with as few responses as possible). These premises are not the only ones which might be applied, nor are they necessarily the best, but they do make possible a logical or rational account of the kind of behavior which the normal subject "ought" to develop, and in this sense the premises might prove useful in the analysis of behavior in "abnormal" subjects.

The following predictions can be made:

1. Fixed ratio. When a fixed number of responses must be made per reinforcement, effort cannot be minimized (i.e., the number of responses is fixed.) but rewards can be maximized by responding rapidly.

2. Fixed interval. When a fixed time interval must elapse before a single response will produce a reinforcement, rewards can be maximized by responding more frequently as the end of the interval approaches (thus assuring delivery of reward as soon as possible) and effort can be minimized by not responding during the initial part of the interval.

3. Variable interval. When an irregularly varying time interval must elapse before a single response will produce reinforcement, rewards can be maximized by maintaining a rate which insures delivery of each reinforcement as soon as possible and effort can be minimized by not developing rates which exceed the optimum rate.

4. Nonreinforcement, extinction, S$^{\Delta}$. When responses are not reinforced, rewards cannot be maximized, but effort can be minimized by responding infrequently or not at all.

Food-deprived, food-reinforced pigeons and money-reinforced humans were tested under comparable multiple schedule conditions. There were repeating cycles of four components occurring in the same sequence in each cycle: FR, fixed ratios of 20; FI, a fixed interval of 60 seconds; VI, variable intervals averaging 15 seconds (range of 0-30 seconds); and S$^{\Delta}$, a 60-second nonreinforcement period. The ratio and variable interval components were present for 60 seconds in each cycle, and changes to the next component were made independently of reinforcements. The discriminative stimuli, colored lights, were green (FR), yellow-orange (FI), red (VI), white (S$^{\Delta}$). The pigeons pecked a key. The humans pressed a metal lever. The pigeons' reinforcement consisted of a 4-second access to grain. The humans' reinforcement consisted of pennies, which were recorded on a counter. In some cases with humans multiple-reinforcements were used: nickles, projected nude slides, or "points" on a counter.

Figure 1 presents data in the form of bar graphs. Each bar graph represents a single 40-minute session. Each bar presents the mean re-

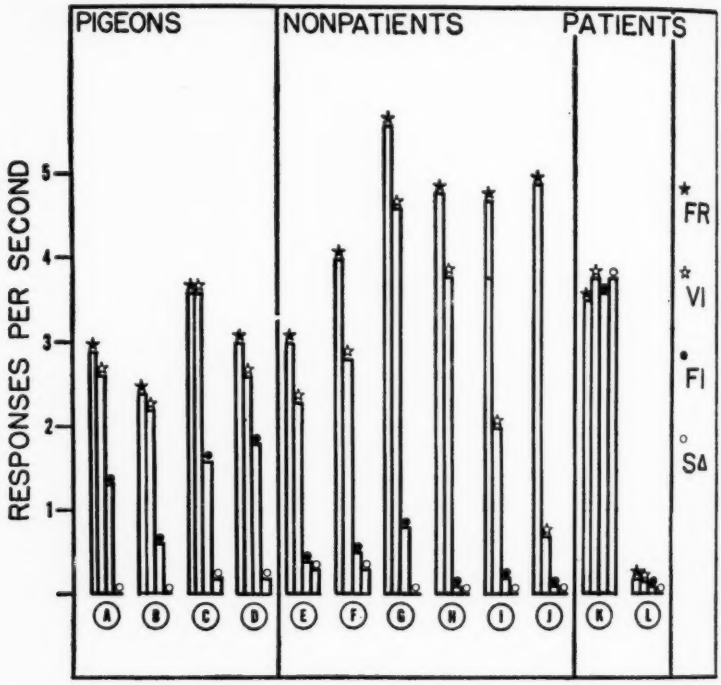

Figure 1. Mean responses per second for each of four components during a single multiple schedule session.

sponses per second rate for a given component for all 10 cycles constituting a single session.

Pigeons. Two pigeons with extensive prior conditioning in the situation were given two successive sessions separated by a 1-2 minute timeout. Bar graphs A and B are for one pigeon, C and D for the other. Some "satiation" is evident: the overall rate for each bird was somewhat lower in the second session. The fixed ratio rates for both pigeons were lower than usual due to malfunctioning of the key. The SΔ responding in C and D reflected "superstitious" behavior apparently due to the fact that the SΔ was of fixed duration.

Nonpatients. Bar graphs E, F, and G are for subjects reinforced with pennies. Bar graphs H, I, and J are for subjects reinforced with

nickels, projected nude slides, or "points" on a counter, with a different reinforcer prevailing throughout any given cycle. Bar graphs E and H are for the same subject tested at two different phases of the research. Bar graphs F and I are for another subject tested at two different times.

Patients. Bar graph K is for a young adult female outpatient (diagnosed chronic ambulatory schizophrenia) who was reinforced with pennies. Bar graph L is for a young adult male inpatient (diagnosed chronic undifferentiated schizophrenia) who received multiple reinforcers.

Each human subject had had at least 5 sessions in the situation prior to the one from which the data in Figure 1 was taken.

With reference to the data of Figure 1, the following observations may be of interest:

1. The pigeons and nonpatient subjects yielded analagous behavior with respect to the rank-ordering of components in terms of mean overall response rates: i.e., fixed ratio first, variable interval second, fixed interval third, and nonreinforcement fourth. Generally speaking, the performances of the pigeons and the nonpatients were indistinguishable with regard to the fixed ratio, variable interval, and nonreinforcement contingencies. Performance in the fixed interval component was significantly different. The pigeons exhibited the typical initial pause and subsequent scalloping (Ferster & Skinner, 1957). The nonpatients (and patient L) exhibited long initial pauses followed by an immediate shift to a stable terminal rate.[7] The humans were not permitted to take their watches into the cubicle, and all reported developing some "counting" system for timing the interval (excepting patient K). Furthermore, during the initial cycle of their early sessions the nonpatients reported responding throughout the fixed interval for the purpose of checking on their counting system, and also responded in S$^{\Delta}$ for the purpose of determining whether that component had been altered. With repeated testing, this behavior tended to disappear, but it was present for E, F, and G and resulted in somewhat elevated FI and S$^{\Delta}$ values.

2. Neither patient produced performances comparable to those of the pigeons and nonpatients. Patient K simply responded at about the same rate throughout each session (the data are from her 10th session).

[7] Similar results have been reported recently by Stoddard, Sidman, and Brady at the 1960 meetings of the Eastern Psychological Association.

Patient L responded at a very low overall rate, but his performances were related to the components in the same general manner as occurred with the pigeons and nonpatients: i.e., multiplying his rates by a constant would produce a bar graph quite similar to theirs.

In summary, when tested under comparable multiple schedule conditions in a free operant response situation, pigeons and "normal" adult humans yielded analagous performances with respect to the overall response rates associated with specific reinforcement contingencies, whereas two psychiatric patients yielded clearly deviant performances. With the exception of fixed intervals, the behavior of pigeons and nonpatients was comparable.

THE PRESENCE OF THE EXPERIMENTER

An important feature of human operant experiments is the absence of the experimenter from the subject's immediate environment. In the course of these investigations, data were obtained accidentally from one subject which suggest that the presence of the experimenter can exert a definite effect upon performance. Figure 2 Panel A presents cumulative response curves for two successive sessions held on different days about one week apart. The subject, an adult male hospital employee, was being reinforced with pennies for every 20th leverpressing response. The experimenter was absent from or present in the experimental room at various times during the two sessions, as indicated by A (absent) and P (present). When present, the experimenter could not be seen, but his presence was known to the subject. In the first session, the experimenter's presence was associated with a definite decrease in response rate. By the end of the next session, however, this effect had disappeared. At the open arrowhead, a malfunction in the programmer terminated with extinction.

THE EFFECTS OF DIFFERENT REINFORCERS

The analysis of "motivation" involves, in part, the determination of the ways in which different "incentives" (that is, different kinds of reinforcing stimuli) control behavior. In the free operant response situation, such analyses take the form of manipulating types of reinforcers. Thus, for example, Lindsley (1956) compared the effects of candy, projected nude slides, nickels, and "feeding a kitten" upon the response rates of chronic psychotic patients, and Bullock (1959) has reported on the differential responding of adult male and female subjects to pro-

jected nude slides and landscapes. Some examples are given below of effects produced by different reinforcers:

1. A 7½-yr.-old female outpatient (being treated with play therapy for educational-emotional difficulties) was conditioned to press a lever for various reinforcements. In the course of a number of sessions involving a fixed ratio of 20 responses per reinforcement she was sometimes given plastic beads and trinkets (B) and sometimes pennies (P). Figure 2, Panel B presents cumulative response curves for five successive sessions. Plastic beads and trinkets were apparently more effective as

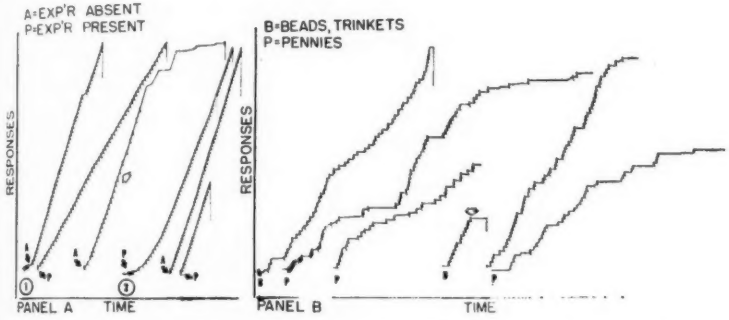

Figure 2. Panel A. Cumulative response curves for two successive sessions of a subject leverpressing for pennies on a fixed ratio of 20. Panel B. Cumulative response curves for five successive sessions of a subject leverpressing for either plastic beads and trinkets or pennies on a fixed ratio of 20. Open arrowhead, Panel A, indicates progressive malfunction of programmer leading to extinction. Open arrowhead, Panel B, indicates a brief intermission.

reinforcers than were pennies: the former produced higher overall rates. The open arrowhead indicates a temporary interruption in a session. This child's mother allowed her to keep the beads and trinkets, but required her to put the pennies in a bank, which may account for the differences obtained.

2. Three young adult male student nurses and one young adult male psychiatric inpatient (diagnosed chronic undifferentiated schizophrenia)[8] were tested repeatedly (1-3 times per week) with a modified form of the multiple schedule described earlier. Each session consisted of seven 4-min. cycles. Each cycle consisted of the same sequence of these four components: (a) 1-min. of FR-50, (b) FI-56 secs., LH-4 sec.[9], (c) 1-min. of VI-17 secs. (range 0-30 secs.), and (d) 1-min. of nonreinforcement. Each experimental date involved three successive 7-

[8] This was subject L in Figure 1. With special experimental treatment, including instructions, this subject's performance was changed from the low overall response rate of Figure 1 to a much more "normal" one.

[9] LH designates limited hold. The actual schedule was one in which a response made during the period 56-60 secs. from the start of the fixed interval component produced a reinforcement. If the subject waited longer than 60 secs., he lost the reinforcement and the next component was presented.

cycle sessions. The reinforcers were as follows: nickels—1st, 4th, 7th cycles; projected nude slides (4-sec. duration each projection)—2nd, 5th cycles; "points" on a counter—3rd, 6th cycles. Figure 3 presents data for all four subjects (the patient is indicated by PAT). The total number

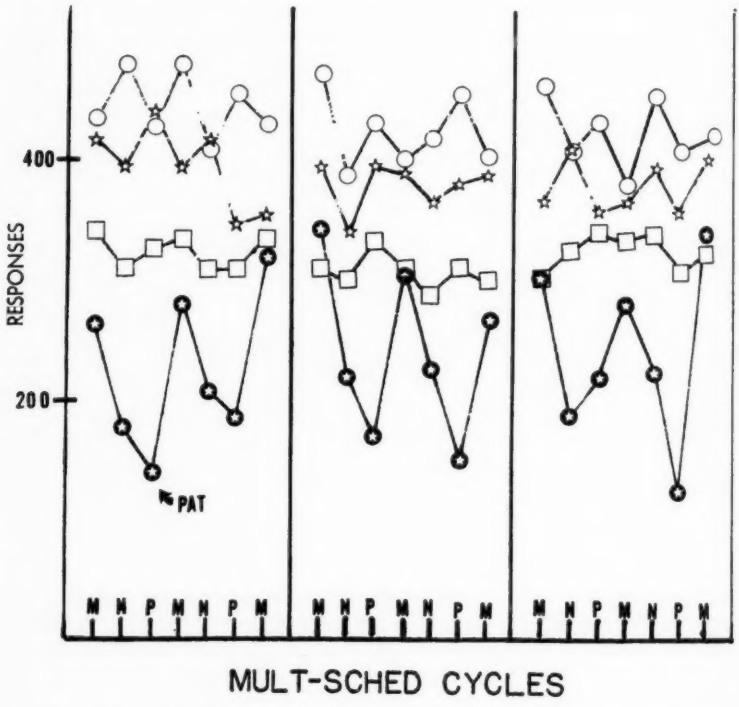

Figure 3. Total leverpressing responses per 4-min. multiple schedule cycle for three young adult male student nurses and one young adult male psychiatric inpatient (chronic undifferentiated schizophrenia), PAT, during three successive sessions of a single experimental date. Reinforcements were nickels (M), projected nude slides (N), or "points" on a counter (P).

of responses per cycle during the three sessions constituting an experimental date are plotted separately for each subject. The reinforcers associated with each cycle are designated by M (nickels), N (nudes), and P (points). Note that though the three nonpatients maintained roughly equivalent overall response rates per cycle throughout, the patient exhibited a definite effect of the reinforcers: responding most rapidly for nickels, least rapidly for points. All four subjects maintained performances appropriate to the specific components: i.e., fixed ratio rates highest, variable interval next, fixed interval next, and nonreinforcement

lowest. The data shown in Figure 3 were taken from sessions which were preceded by several sessions under the same conditions. Throughout a specific project involving a total of 6-12 experimental dates per subject, the student nurses maintained equivalent rates for each reinforcer, whereas the patient showed a progressive changes from equivalent rates to the type of performance reflected in Figure 3.

3. A 40-yr.-old female inpatient (diagnosed chronic undifferentiated schizophrenia, with lobotomy) was tested 3-5 times per week under conditions in which the operant was knobpulling (Bullock & Brunt, 1959) Initially, the reinforcement was a mixture of candy and cigarettes on a continuous reinforcement schedule (every response reinforced). Subsequently, poker chips worth money were introduced, also on continuous reinforcement, with each reinforcement worth a nickel. Continuous reinforcement was then changed to progressively increasing fixed ratios, and, finally, the value of reinforcements was changed to a mixture of 1¢, 2¢, and 5¢ chips. Figure 4, Panel A presents the results in terms of the total number of responses and the total number of reinforcements per session. Note that whereas the candy-cigarettes mixture failed to generate high response rates, the "tokens" brought about a considerable increase in responding.

In summary, data have been presented which indicate that the presentation of different types of reinforcing stimuli to human subjects in a free operant response situation can produce significant effects upon performance.

PSYCHOPATHOLOGICAL INFLUENCES

A number of studies have reported data which indicate that behavior in free operant response situations is subject to psychopathological influences (e.g., King, Merrell, Lovinger, & Denny, 1957; Mednick & Lindsley, 1958; Bullock & Maline, 1958; Bullock & Brunt, 1959; Bullock, 1960). By way of illustrating the kinds of differences which can be obtained between "normal" subjects and psychiatric patients, Figure 4, Panel B presents data on the performances of 16 male and female student nurses and nine male and female psychiatric inpatients. The subjects pulled a knob for monetary reinforcement: poker chips which the patients exchanged for credit vouchers with which they could make purchases in the hospital Snack Bar and which the nonpatients exchanged for cash (Bullock & Brunt). Each subject was given a series of brief tests: (a) 50 continuous reinforcements—each right knob response produced one chip; (b) 50 reinforcements on fixed ratios—the values of the fixed ratios changed progressively from one to 10; (c) discrimination—

alternations between two contingencies, one with the right knob lighted and right knob responses reinforced on fixed ratio and the other with the left knob lighted and neither knob reinforced; and (d) fixed intervals –right knob responses reinforced on a fixed interval of 17-sec.

Figure 4, Panel B, DISCRIMINATION presents the median number of right and left knob responses per 1-min. no reward period for

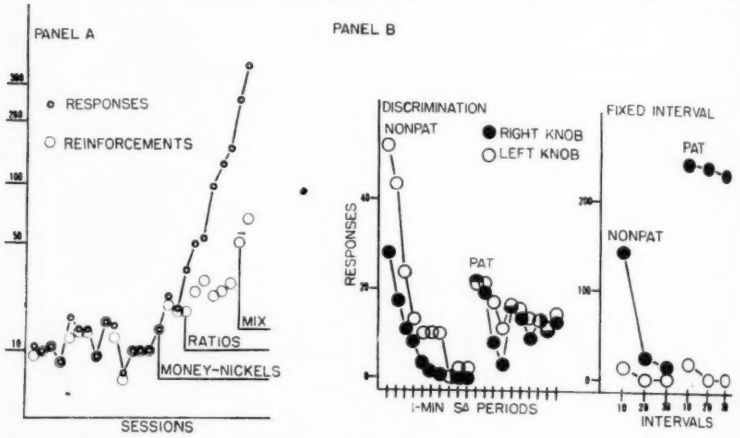

Figure 4. Panel A. Total responses and total reinforcements per session for a 40-yr.-old psychiatric inpatient (chronic undifferentiated schizophrenia with lobotomy) knobpulling for (a) candy and cigarettes on a continuous reinforcement schedule (initial sessions), then (b) poker chips worth 5 cents on a continuous reinforcement schedule (MONEY-NICKELS), then (c) 5 cent chips with progressively increasing fixed ratios (RATIOS), and finally (d) a mixture of 1 cent, 2 cent, 5 cent chips on fixed ratios (MIX). Panel B. Median responses on right and left hand knobs for a group of 16 male and female student nurses (NONPAT) and a group of nine psychiatric inpatients (PAT) in two successive tests involving monetary reinforcement. The DISCRIMINATION panel presents data for 10 1-min. S delta (nonreinforcement) periods. THE FIXED INTERVAL panel presents data for 3 blocks of 10 17-sec. fixed intervals per block.

the non-patients (NONPAT) and patients (PAT). Note that the nonpatient group exhibited a marked decrease in responding on both knobs, with a predominance of left knob responses during the left knob lighted no reward periods. The patient group showed only a slight decrease and much less predominance of left knob responding. Bullock (1960) has reported a subsequent experiment in which this result was confirmed: i.e., nonpatients were more likely to show decreased response rates and predominance of left knob responses when given a similar brief visual discrimination test.

Figure 4, Panel B, FIXED INTERVAL presents the median number of right and left responses per block of 10 17-sec. fixed intervals for the nonpatient and patient groups. Note that the nonpatient group showed a marked decrease in response rate on the right knob, thus evidencing a temporal discrimination, whereas the patient group failed to show such

a decrease in right knob rates. The two groups were comparable with respect to their left knob response rates.

In summary, data have been presented which illustrate the kinds of differences which can be obtained between nonpatient and psychiatric patient subjects in a free operant response situation.

PSYCHOPHARMACOLOGIC INFLUENCES

Dews & Morse (1958) have reported on the effects of dextroamphetamine (Dexedrine, SK&F) upon free operant responding in young adult male humans. In their experiment, a procedure known as differential reinforcement of low rates, DRL, was used, in which (a) the subject had to wait at least N seconds (2.5 or 25 secs.) between successive responses, and (b) the subject had to cumulate N "correct" responses (100 or 10) in order to obtain monetary reinforcement. Under these conditions, d-amphetamine produced a slight but significant decrease in interresponse times: the subjects did not wait as long between responses as they did under placebo conditions.

Figure 5 presents data which illustrate the manner in which drugs can alter performance in a free operant response situation.[10] The subject, a young adult male student nurse, was tested 2-3 times per week under multiple schedule conditions. The multiple schedule involved repeating cycles in which four components occurred in the same sequence: (a) FR-100, a fixed ratio of 100 responses; (b) DRL-25 secs.: the subject had to wait at least 25 secs. before responding; (c) VI-26 secs.: a variable interval of 25 secs. (range 0-60 secs.) in which the subject was requested to respond at a rate of once per second; and (d) S^{Δ}-20 secs.: a 20-sec. nonreinforcement period during which the subject could report on a dictating machine any changes he noticed in his "feelings." The change from one component to the next occurred with each reinforcement (excepting, of course, the fixed duration S^{Δ} period). If, in the DRL component, the subject responded too soon, he was given another opportunity. In Figure 5, these occasions are designated by vertical dashed lines. The response was pressing a Lehigh Valley Electronics pigeon key, which was mounted in a small box which the subject held on his lap. The reinforcement was money (approx. $2.40/hr.). After several preliminary training sessions, the following schedule was introduced: placebos, methamphetamine (Amphedroxyn, Lilly, 5 mg), phenylglycodol (Ultran, Lilly, 600 mg), methamphetamine (5 mg), phenylglycodol (300 mg), placebos. The placebos or drugs were administered orally at the start of the 1-hr. sessions.

[10] This data was reported originally in a paper, "Human applications of drug-behavior profiles," read in a symposium of the Psychology Section, Amer. Assoc. Advance. Science annual meeting, 1958.

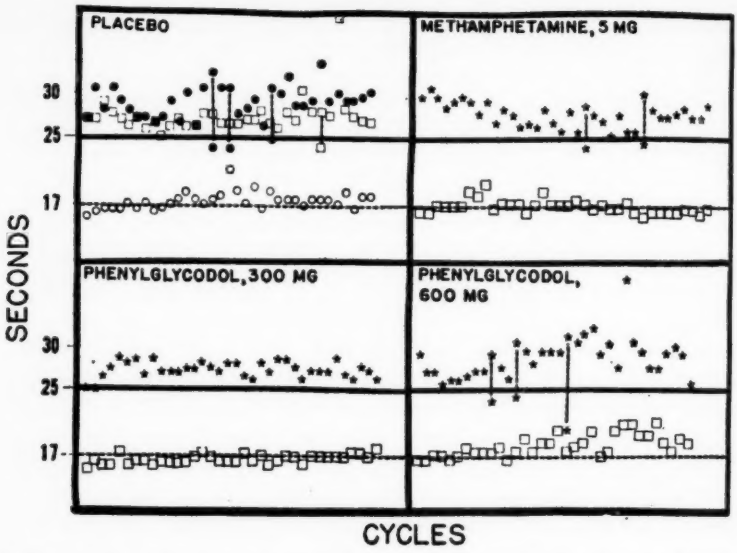

Figure 5. Pause duration in seconds per DRL-25 secs. component (upper points in each panel) and duration in seconds of FR-100 component (lower points) for a young adult male student nurse pressing a pigeon key for monetary reinforcement. Data are shown for four different sessions of 1-hr. duration each. Placebos or drugs were administered at the start of each session. See text for details.

Figure 5 presents the data for the DRL-25 secs. and FR-100 components. The data shown are for the initial placebo and methamphetamine sessions and both phenylglycodol sessions.

DRL-25 secs. The measure used for the DRL component was the time in seconds between the start of the DRL component, which was signalled by a colored light, and the first response. The upper points in each panel present these values. The open squares in the placebo panel are from the final placebo session. Generally speaking, the performance in the placebo sessions was characterized by a slight but consistent increase in the "pauses": the subject waited somewhat longer in the second half of the session. Methamphetamine produced either shorter "pauses" in the second half of the session (as shown in Figure 5) or no change in "pauses," whereas 300 mg of phenylglycodol did not alter "pauses" and 600 mg produced a marked increase. There was some progressive improvement in DRL performance throughout the sessions, and it is the author's impression that when DRL responding is continuously reinforced (as in this study), rather than intermittently (as in the Dews & Morse study) its sensitivity to drug effects may be somewhat lessened.

FR-100. The measure used for the FR component was the time in seconds required for the subject to emit the 100 responses. The

lower points in each panel present these values. The placebo performances were consistently characterized by a slow drift from values of about 16 secs. to values of about 18 secs. Methamphetamine consistently altered this drift: the subject was responding with values of about 16 secs. at the end of both methamphetamine sessions. Phenylglycodol at 300 mg produced a performance intermediate to placebos and methamphetamine, whereas the 600 mg dose brought about a marked slowing in the rate at which the subject emitted his ratios.

In summary, data have been presented which illustrate the manner in which free operant responding can reflect the effects of both stimulant and tranquilizer drugs.

SUMMARY AND CONCLUSIONS

The problem of controlling and predicting voluntary behavior as a necessary condition for a science of behavior has been discussed in relation to the conception that present voluntary responses reflect the effects of past experiences, or learning. The use of free operant response concepts and techniques for studying voluntary behavior in animals and humans has been examined. The characteristics of free operant response situations have been described and illustrations presented of various aspects of operant behavior in human subjects, including (a) the performances of pigeons and humans under comparable multiple schedule conditions, (b) the effects of the experimenter's presence, (c) the effects of different reinforcers, (d) psychopathologic influences, and (e) psychopharmacologic influences.

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"SOCIAL-RELEASER MECHANISMS" IN BIRDS — A CONTROLLED REPLICATION OF TINBERGEN'S STUDY

MALCOLM A. McNIVEN

E. I. du Pont de Nemours & Company, Inc.

In the last 10 years, publications discussing human and animal learning have quoted the studies of the European ecologists (Lorenz, Tinbergen, Baerends, et al.) with increasing frequency and with increasing importance attributed to their work (Beach, 1951; Hinde, 1959; Miller, 1951; Nissen, 1951). In general, the studies have supported the thesis of the existence of inherited neural mechanisms which control behavior.

Tinbergen's work presents an interesting hypothesis—the concept of the "social releaser." The social releaser is an innate neural releasing mechanism which, when acted upon by certain adequate stimuli in the natural environment, triggers-off certain adaptive responses. These innate releasing mechanisms are acquired by the organism through a process of species evolution and manifest themselves in such behavioral segments as mating, fright reactions, threat behavior, and other such stereotyped active patterns. Due to the fact that there is considerable generalization of response to various classes of stimuli, the resulting behavior may appear abortive and non-adjustive, as in the case of the dragonfly laying her eggs on a tar roof, or the threat behavior of the three-spined stickleback on seeing his image in the mirror. Nevertheless, the great majority of responses are appropriate and appear when the proper sign stimulus is perceived by the organism. The social releaser, which is a specialized releaser, is a materialization of the innate releasing mechanism which, through adaptive evolution, has persisted because of its appropriateness. The implications of this hypothetical mechanism are interesting when extended to the subject of nativism in human behavior, as Tinbergen does.

A study frequently referred to is that of Tinbergen's (1948) which reported the reactions of several species of birds raised in isolation to an ambiguous silhouette which resembled a predator when moving in one direction and non-predator when pulled in the other direction. Hilgard reports this study in his chapter in *Perception—an Approach to Personality* (1951); Miller reports the study in his discussion of the acquisition of fear in the *Handbook of Experimental Psychology* (1951).

In an effort to verify these rather startling findings, a series of controlled field studies was conducted to determine the frequency of occurrence of this response and its generality to other species of birds.[1]

PROBLEM

The general hypothesis to be tested was that birds raised in isolation will react differently to an ambiguous silhouette when it is presented to them in different directions (e.g., short neck leading vs. long neck leading). When stated in the null form:

H_0: There will be no differences between the number of escape responses elicited upon presentation of the silhouette in the short-neck direction and in the long-neck direction.

The sailing and swooping variable was included since Tinbergen found that swooping of a stimulus more often elicited an escape response than did the sailing response. A second null hypothesis may be stated:

H_0^1: There will be no differences in the proportion of escape responses elicited by the swooping and the sailing presentation of the silhouette.

METHOD

The experimental treatments of all groups tested are shown in Table 1.

TABLE 1

GROUP TREATMENTS

		Group					
		IA	IB	IC	II	III	IV
Short-neck Leading	Sailing	X		X	X	X	X
	Swooping		X	X	X	X	X
Long-neck Leading	Sailing	X		X	X	X	X
	Swooping		X	X	X	X	X

Subjects

The responses of domestic chickens (New Hampshires), undomesticated ducks (mallards), and undomesticated pheasants were observed in the experimental situation. These groups are described in Table 2.

[1] These studies were initiated in the Comparative Psychological Laboratory at Ohio University under the guidance of J. R. Patrick. They were continued at The Pennsylvania State University under the guidance of C. R. Carpenter and with the cooperation of the Department of Poultry Husbandry.

TABLE 2

INFORMATION ABOUT EXPERIMENTAL GROUPS

Group	Type Bird	*N*	Tested Individually or in Groups	Age	Number of Trials per Subject
I A	Chicken	10	Group	7 weeks	12
I B	Chicken	10	Group	7 weeks	12
I C	Chicken	5	Individually	7 weeks	4
II	Chicken	8	Individually	6 weeks	24
III	Duck	5	Individually	4 weeks	24
IV	Pheasant	24	Individually	4 weeks	24

Apparatus

A wire enclosure 10 feet in diameter was used as the test area. This was located close to the area where the birds were housed. A wire was strung over the enclosure at a height of 15 feet.

The silhouettes used in the experiment were similar to those described by Tinbergen. They were constructed of Masonite; one was painted black and the other was the unpainted brown color. The black silhouette used originally had a 20" wingspan and a 10" body. The unpainted silhouette used later was larger with a 28" wingspan and a 14" body. The design of these silhouettes was such that when they were pulled in the direction with the short neck leading and the long body behind, they resembled a hawk; but when pulled in the opposite direction, the silhouettes resembled a goose. (See Figure 1.)

Procedure

The animals were kept in a relatively shadow-free room from the time of hatching until the experimental trials. When placed in the wire enclosure, they were allowed a ten-minute orientation period. The silhouette was then sailed over them on the wire, first in one direction then in the reverse direction. Order was rotated. In the sailing position of the design each group received six presentations of the silhouette in each direction or 12 trials in total. In the swooping portion, the same procedure was followed as above. The swooping trials always followed the sailing trials, except in the case of 10 chickens in Group IB who received only swooping trials and in Group IA which received no swooping trials. The swoop was achieved by fixing a 12-foot wire to the wire over the enclosure and then holding the silhouette up and allowing it to swoop over the enclosure and up where it was held for the next trial. Responses of the subjects were coded as follows: (1) escape response, (2) crouching response, (3) no response.

Figure 1. Design of silhouette used.

RESULTS

Table 3 shows the proportion of escape and crouching responses combined for each group to the hawk and goose silhouette.

TABLE 3

PROPORTION OF ESCAPE RESPONSES TO DIRECTION OF STIMULUS MOVEMENT

	Base	Short-neck Leading	Long-neck Leading
Group I A	120	.83	.00
I B	120	31.67	33.33
I C	20	37.50	32.50
II	192	10.42	10.42
III	120	13.72	11.76
IV	576	43.26	42.90
Total	1148	32.50	32.26

TABLE 4

PROPORTION OF ESCAPE RESPONSES TO TYPE OF MOVEMENT

	Base	Sailing	Swooping
Group I A	120	.83	—
I B	120	—	64.16
I C	20	.00	65.00
II	192	5.21	15.63
III	120	7.40	18.75
IV	576	7.61	77.08
Total	1148	5.29	59.29

The results shown in Table 3 when tested by a Chi-square test indicated no differences in the number of escape responses elicited by the silhouette when pulled in the short-necked direction and when pulled in the long-necked direction. The null hypothesis H_0 is not rejected.

The results in Table 4 indicated that there were significant differences between the escape responses elicited by the swooping silhouette vs. the sailing silhouette. Thus H_0^1 is rejected ($P=.001$.)

DISCUSSION

It is apparent from the results in Table 3 that the birds did not respond differentially to the shape of the silhouette when it was moved in the "long neck leading" and the "short neck leading" directions. One would conclude without question that this variable was not a factor in the birds' responses to the silhouette. It should also be noted that this finding held true for all the experimental groups.

It is equally apparent from Table 4 that the variable of type of movement was an important factor in determining the birds' responses.

The fact that none of the experimental groups was tested at various ages leaves unanswered the question of the effect of age on the evocation of a selective response to "predator" shapes. Some of Tinbergen's birds exhibited the selective response at 4 weeks. Therefore the age of our experimental animals (4-7 weeks) should have been adequate to illustrate the phenomenon.

Hirsch, Lindley and Tolman (1955) report that, when Tinbergen's hypothesis was tested under laboratory conditions with white Leghorn chickens, the form of the silhouette (changed by direction of movement) was not a significant source of variance. Tinbergen's results concerning a hypothetical "social releaser mechanism" were not supported in this study. The experimental techniques used by Hirsch, Lindley and Tolman were quite similar to those used in the present

study, except for the fact that their trials were run indoors under controlled background and illumination conditions while the present study was conducted outdoors under natural conditions.

The results in Table 3 agree with those results reported by Hirsch, Lindley and Tolman.

The data presented in Tables 3 and 4 certainly bring into question the results of the Tinbergen study. However, one must ask: "How could these results differ so markedly from those obtained by Tinbergen?" Perhaps the answer lies in the different approaches to studying a problem demonstrated by American comparative psychologists and their European counterparts. Tinbergen's statement of the results merely indicated that young gallinaceous birds, ducks, and geese reacted in a frightened manner to the silhouette when moved in the "hawk" direction, but showed no escape responses when the silhouette was moved in the "goose" direction. No data were reported and no description of the details of the procedure was reported. A request to Tinbergen for specific information about the experimental procedure was not answered. The answer to the question of why the results differ may lie in the details of procedure, in the details of the subjects' environment prior to the trials, or in many other aspects of the study. But, unless these are reported by the experimenter, we will never know. Writers in America have given wide circulation to the studies of the Lorenz-Tinbergen school without demanding corroborative evidence of the adequacy of the experimental work. Several individuals have worked closely with members of the Lorenz-Tinbergen school and have reported on their work. One such report by Lehrman (1953) criticized Lorenz's theory of instinct for the following reasons:

1. It is rigidly canalized by the merging of widely different kinds of organization under inappropriate and gratuitous categories.
2. It involves preconceived and rigid ideas of innateness and the nature of maturation.
3. It habitually depends on the transference of concepts from one level to another, solely on the basis of analogical reasoning.
4. It is limited by preconceptions of isomorphic resemblances between neural and behavioral phenomena.
5. It depends on finalistic, preformationist conceptions of the development of behavior itself.
6. As indicated by its applications to human psychology and sociology, it leads to, or depends on, (or both), a rigid, preformationist, categorical conception of development and organization.

There is no doubt that the work of the European ecologists and ornithologists will have and should have heuristic value for studies

of animal and human behavior in the U.S. However, we should not accept the conclusions of the ecologist's studies until they have been replicated by more experimental techniques or until the details of the experimental procedures, equipment, and data have been fully reported.

CONCLUSIONS

1. Chickens, ducks and pheasants fail to exhibit the escape responses to the sign-stimulus of "short-neckedness" as reported by Tinbergen.

2. Chickens, ducks, and pheasants do exhibit escape responses to a swooping movement of the silhouette, but this is true regardless of the direction in which it is moving.

3. The slow sailing of the silhouette over chickens, ducks, and pheasants does not elicit escape responses, regardless of direction.

SUMMARY

Tinbergen reported a study which found that young gallinaceous birds, ducks, and geese reacted in a frightened manner to a silhouette when it moved in the "hawk" direction, but showed no escape responses when the silhouette was moved in the "goose" direction. In order to replicate this study, chickens, ducks, and pheasants were presented with the same type of silhouette used by Tinbergen. A total of 1178 trials were run with the three species of birds. The silhouette was presented in a sailing manner (moving horizontally over the birds), and in a swooping manner. The silhouette was presented alternately in one direction then the other. The results do not support the data reported in Tinbergen's study. There was no difference in the number of escape responses to the silhouette when it moved in the two directions.

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RATING OF PATIENTS BY MENTAL HOSPITAL WARD ATTENDANTS[1]

HERBERT GERJUOY
University of Toledo

B. G. ROSENBERG, JOSEPH K. BALOGH
Bowling Green State University

ROBERT McDEVITT and JAMES G. BOND
Dayton Children's Psychiatric Hospital *Toledo State Hospital*

The present investigation is part of a larger study (Bond, et al., 1960) that dealt with the applicability of milieu therapy to chronic regressed female schizophrenics. In the larger study, a six month long program of intensive milieu therapy was carried out on a specially selected ward group. The attendants on the experimental ward were subjected to intensive indoctrination in techniques for dealing with patients, with particular emphasis on techniques for encouraging patients to substitute social interaction responses for their habitual autistic withdrawal. A major part of the indoctrination program was a series of weekly group meetings of the ward attendants with the ward professional staff. The ward psychiatrist and the ward psychiatric nurse cooperated in demonstrating to the attendants superior techniques for dealing with patients without recourse to punitive and repressive measures such as physical restraint, isolation, etc.

In the course of the study it was noted that some attendants became involved in unusually intense personal interactions with certain patients. The records of the weekly group meetings of the attendants with the ward professional staff showed that an increasing proportion of the time was spent discussing the relationships between a few attendants and a few patients. It was felt that not all attendants perceived the patients in the same way, that a minority had developed idiosyncratic perceptions. It appeared that the special attendant-patient relationships might be important in determining the response of patients to the therapeutic program and it appeared that particular patients were improving because they had been "adopted" by certain ward attendants while other patients were having their illness complicated by iatrogenic disorders arising from unfortunate interactions with certain ward attendants. This idea was consistent with the findings of

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Abrahams (1948), Galioni, Adams, and Tallman (1953), Greenblatt, et al. (1955, 1957), Martin (1950), Rowland (1938, 1939), Sanders, Fitzgerald, et al. (1959), and Schwartz, et al. (1950, 1953, 1956).

The following general question was considered: Did certain aides have idiosyncratic attitudes about certain patients? This question was investigated using a new clinical rating scale developed for and tested by this study. Using this instrument, the Attendants' Rating Scale (ARS), each attendant rated each individual patient.

The new scale permitted investigation of whether attendants' perceptions of patients were consistent with other measures of the patients' clinical statuses. In this study, the ARS ratings were compared to the clinically validated Psychiatric Rating Scale[2] (PRS) ratings of the same patients. If attendants were accurate in their ratings of patients, then attendants' ratings would be a less expensive and less time consuming alternative to professional evaluations such as the PRS. Even if the attendants' ratings were not as valid as professional evaluations, they might still be useful to professional staff members who were making an ultimate evaluation.

In the course of this portion of the larger study, the following specific questions were considered:

1. Were there differences among attendants in their ratings of patients?
2. Were all patients rated equally favorably or unfavorably?
3. How reliable were the ARS ratings?
4. Using the PRS ratings as criteria, how valid were the ARS ratings?

METHOD

Patient Sample

The 61 patients in the experimental sample were drawn from the custodial wards for chronic female patients at Toledo State Hospital. To make up the sample, patients were examined by two psychiatrists, the Director of Research and Training and the psychiatrist in charge of the experimental ward. Patients were included in the sample if they had all of the following qualifications:

1. Clinically and historically verified diagnosis of schizophrenia made independently by two psychiatrists,
2. Absence of history of physical findings suggesting organicity or mental deficiency,[3]
3. Severe withdrawal and socially isolated behavior, manifested by persistent mutism, negativism, incontinence, assaultiveness, self-destructive tendencies, and overtly bizarre behavior,

[2] The Psychiatric Rating Scale was developed by Malamud and Sands for use with schizophrenics who had been lobotomized. It measures behavior at the lowest level of regressed functioning. This measure has been used in a number of studies and has proven reliable and valid (Malamud & Sands, 1947). It was used by Galioni, Adams, & Tallman (1953) in their study of milieu therapy.

4. Relative refractoriness to treatment, including electro-shock therapy, insulin, and the phenothiazines,

5. Age 62 years, or below,

6. Hospitalized for at least a continuous two year period prior to examination without change in clinical status.

Attendant Sample

The nine attendants[4] who were to work in the milieu therapy program were selected by the nursing supervisor as "potentially good workers." The nursing supervisor was not informed of the details of the special duties for which the attendants were recruited. Seven of the attendants assigned to the research ward were already working on that ward prior to the start of the project.

Milieu Therapy Program

The six month milieu therapy program is described elsewhere (Bond, et al., 1960). In addition to the attendant indoctrination procedures referred to above, the program included ward redecoration, reduction of the ward population by about one fourth, not allowing patients to stay in bed by day, occupational and recreational therapy programs, weekly ward visits by specially trained volunteer workers, and off-ward social and recreational activities. Phenothiazine drug dosage was held at minimal or zero level, and all other medical therapies were discontinued, including night sedation.

Psychiatric Rating Scale

The PRS was administered before and after the experimental period by four senior staff psychiatrists familiar with its use, but not connected with the major investigation. On each occasion, each psychiatrist rated 15 or 16 patients. As a control measure, at the end of the experimental period, each psychiatrist rated a different randomly selected set of patients.

Attendants' Rating Scale

Interviews with attendants revealed three distinct areas in which attendants differed in their perceptions of particular patients. A three item rating scale was devised by which the attendants rated each of the 61 patients on the ward. It was administered shortly after the PRS ratings were completed. The items dealt with:

1. How time consuming it was to work with the patient,

2. Whether the attendant perceived the patient as suitable for the milieu therapy program,

3. How pleasant it was to work with the patient.

[3] This criterion excluded patients who had undergone brain surgery. By error, two patients were included in the sample who had previously undergone therapeutic prefrontal lobotomies.

[4] Midway through the six month long milieu therapy program, one of the attendants was replaced because she had to transfer to another ward when her husband changed his working hours. Both the attendant who transferred away and her replacement participated in ratings of the patients. Thus, there were ten attendants in the attendant group considered in this report.

At the close of the six month milieu therapy experiment, the ARS was completed by the ten attendants who had worked regularly on the experimental ward. Each attendant was asked to read the following preliminary instruction sheet:

> We are considering recommending that other hospitals start projects similar to the one we are now working on. We know that projects of this kind require a great deal of time and energy from our attendants. Other hospitals will not want to try such projects with patients who are not going to improve. With this in mind, we would like the answers to a few questions about the patients who have taken part in this project. The answers to these questions will give us information about the patients who should be used in future projects like this one.
>
> On each page is a list of all patients who took part in this project. For each patient, answer the question on the top of the page. Please complete each page separately before you go on to the next page.
>
> For each question, please score the patients as if you were giving them grades in school.

The attendants were then given three rating scale items in different random orders. Each item consisted of a six column table listing at the left the names of the patients in alphabetical order with the remaining five columns headed A, B, C, D, and F respectively from left to right. Above the table were instructions specific to that item. Each item was returned to the experimenter by the attendant before the next item was given to her. Attendants did their work independently. Most completed their questionnaires at the close of the regular weekly discussion session. Three attendants who were not present on that occasion were sought out while on duty and completed the scale during their regular work period.

For item 1, the instructions above the table read:

> If working with the patient never took time away from other patients, then give the patient a grade of A.
>
> If working with the patient hardly ever took time away from other patients, then give the patient a grade of B.
>
> If working with the patient took time away from other patients about half the time, then give the patient a grade of C.
>
> If working with the patient usually took time away from other patients, then give the patient a grade of D.
>
> If working with the patient always took time away from other patients, then give the patient a grade of F.

Comparable instructions were given for the other two items. As may be noted, the instructions for the items were made sufficiently ambiguous so that attendants would be encouraged to respond in terms of subjective impressions of the patients.

Attendants' ratings were coded for analysis: a rating of A as +2, and B, C, D, and F as +1, 0, −1, and −2, respectively.

RESULTS

Analysis of the results of the milieu therapy program revealed substantial improvement in the patients' clinical statuses, as measured by the PRS scores and a variety of other indices (Bond, et. al., 1960). For the data dealt with in this report, analysis of variance was performed upon the coded ARS ratings of the patients by the experimental ward attendants. Three variables were considered: *patients, attendants,* and *items.* Table 1 presents the results of this analysis, [5,6] including simple effects of the *patients* and *attendants* variables.

TABLE 1

ANALYSIS OF VARIANCE OF CODED ARS RATINGS

Source	Sum of squares	*df*	*F*[a]
Patients (P)	452.74	60	17.68***
Patients, Item 1	372.14	60	14.53***
Patients, Item 2	122.11	60	4.77***
Patients, Item 3	119.33	60	4.66***
Attendants (A)	372.01	9	96.86***
			54.18***[b]
Attendants, Item 1	273.95	9	71.33***
Attendants, Item 2	123.22	9	32.08***
Attendants, Item 3	137.90	9	35.90***
Items (I)	61.22	2	71.73***
			22.84***[c]
P x A	411.96	540	1.79**
P x I	160.85	120	3.14***
A x I	163.05	18	21.23***
error (P x A x I)[a]	460.88	1,080	
Total	2,082.71	1,829	

[a]See note 5. [b]Using (P x A) as error term. [c]Using (P x I) as error term. **Significant at .01 level. ***Significant at .001 level.

[5] As the error term in the analysis of variance, the triple interaction among the three variables was used. This error term is conservative, since it does not allow for the possibility that some triple interaction may be due to a genuine non-random intrinsic effect. Hence, results obtained using this error term may be expected to err in the direction of too few significant findings. Parallel analyses using alternative error terms did not lead to any appreciably different results.

[6] To simplify presentation, only results of the analysis of attendant ratings of patients significant at the 0.001 level are discussed. This reflects the exploratory and tentative nature of conclusions based upon the small sample of subjects.

The interaction between the *patients* variable and the *items* variable was significant ($P<0.001$). This interaction reflected a tendency for the attendants to react differentially to the patients with respect to different items. For example, some patients were perceived as time consuming but nonetheless relatively pleasant to work with, while others were rated as unpleasant but not time monopolizing. There was also a significant interaction ($P<0.001$) between the *items* variable and the *attendants* variable. In other words, not all attendants responded in the same way to the different items. For example, one attendant perceived the patients as relatively pleasant to work with, but not generally the right kind of patients for such a project, while another attendant tended to regard the patients as the right kind for such a project, but unpleasant to work with. Because of the significant interactions, the following simple effects were investigated: (a) differences among patients[7] for each of the three items considered separately, and (b) differences among the attendants[7] for each item considered separately[8].

As can be seen in Table 1, separate tests of the differences among patients for each item revealed significant ($P<0.001$) differences among patients on each of the three items.[7]

Table 1 also presents the results of the tests of differences among the mean ratings by the attendants, taken separately for each item. For all three items there were significant differences among attendants in their mean ratings.[7]

The main effect of *patients* was significant ($P<0.001$), and therefore the hypothesis was rejected that there were no differences among patients in terms of attendants' over-all perceptions of them on all three items combined. Some patients were perceived more favorably overall than others, despite the interaction between *patients* and *items*. Similarly, the significant ($P<0.001$) main effect of attendants implied that some attendants were generally more favorable in their ratings of patients on all three items combined than were other attendants, despite the significant interaction of *attendants* and *items*. Similarly, the main effect of items was significant ($P<0.001$) despite the interactions of this variable with the others. Item 1, pertaining to time monopolization, elicited a mean coded rating of 0.93 (the coded score of 1.00 corresponded to rating the patient as "hardly ever" taking time away from other patients). The responses to Item 1 were significantly less favorable ($P<0.001$) than were the responses to the other two items. The mean coded responses to the other two items did not differ significantly from each other. The mean coded response for these two items combined was 1.31.

7 For the differences revealed by the tests, Tukey tests were performed to group the data into subgroups which were relatively homogeneous within subgroups, but which were significantly different between subgroups. This further analysis is reported by Bond, et al. (1960).

8 Bond, et al. (1960) report an analysis of differences among items for each of the 61 patients considered separately, and an analysis of differences for each of the ten attendants considered separately.

The over-all mean of the coded ARS ratings was 1.19 (moderately favorable). This average rating (pooled over patients, attendants, and items) differed significantly from 0.00 (the midpoint of the scale). The F equalled 604.6, with 1 and 1080 df, significant at the 0.001 level, using the error term of Table 1. Thus the attendants' ratings were, as a whole, favorable.

The two sets of PRS scores were correlated with the three sets of attendant ratings of patients obtained by taking separately Items 1, 2, and 3 of the ARS (pooling ratings of patients by different attendants). These correlations are presented in Table 2, with the correlation between the two PRS scores and the intercorrelations among the rating

TABLE 2

INTERCORRELATIONS[a] AMONG PATIENTS' PRE- AND POST-PROJECT PRS SCORES AND THEIR POOLED RATINGS ON EACH ARS ITEM

Measure	PRS score		Rating scale item rating		
	Pre-project	Post-project	1	2	3
PRS Score:					
Preproject		0.48***	0.44***	0.31*	0.30*
Postproject			0.70***	0.50***	0.62***
Rating Scale Item Rating:					
1. Time consuming				0.64***	0.75***
2. Suitable					0.64***
3. Pleasant					

[a]PRS scores decline with patient improvement, while ARS scores rise. Therefore, scores coded so that positive correlation indicates agreement between scales.

*Significant at .05 level.

***Significant at .001 level.

scale items. For each correlation, the hypothesis was tested that: there was no relationship between the variables correlated. This hypothesis was rejected in each case. Attendants' ratings of patients were highly related to the clinical statuses of the patients as measured by the PRS. They were more highly correlated with the postproject PRS ratings than with the preproject PRS ratings. The correlations between the preproject PRS scores and each of the four measures obtained at the close of the project (the postproject PRS scores and the three sets of ratings) were in each case lower than any of the six correlations among

the four postproject measures. There was only one chance in 210 of the four preproject PRS correlations each falling below the other six under a null hypothesis that all the correlations were equal.

DISCUSSION

The results of the analysis of the ARS ratings supported the conclusion that the attendants viewed patients differentially, and varied among themselves in their general favorable or unfavorable perception of patients. Some patients were viewed more favorably than other patients. Some attendants tended, generally, to be more favorable in their view of patients than other attendants. There was also a tendency for the attendants to react differentially to the different items on the three item rating scale.

The analysis revealed significant interactions among the variables. However, the significant main effects of the *attendants* and *patients* variables were not less important because these variables interacted significantly with the *items* variable. While it is true that differences among attendants in their perceptions of patients differed for different items, the significant main effect of the *attendants* variable implies that there were preponderant differences among attendants in how they viewed their patients over all three items combined; these preponderant differences among attendants were not masked by discrepancies among the findings for the different items. A similar interpretation may be made for the significant main effect of the *patients* variable. Differences among patients were apparent despite any discrepancies among the findings for the different items.

On the other hand, the significant interactions with *items* imply that the three different items tapped different sources of variance in the attendants' perceptions of their patients. While there may have been a halo effect, the *patients* by *items* interaction implies that attendants did not generally respond merely in terms of an over-all favorable or unfavorable set associated with a particular patient, but instead discriminated among the three different aspects of patient behavior rated by the three different items.

The significant simple effects of *patients and attendants* separately (see Table 1) imply that each item by itself discriminated among the patients and attendants, i.e., for each item some patients were rated significantly higher than others, and for each item some attendants' average ratings of patients were significantly higher than other attendants'.

As Table 2 shows, the attendants' ratings of their patients were highly correlated with the clinical statuses of the patients as measured by the relatively objective PRS ratings. The ARS ratings were more closely associated with the postproject PRS ratings than with the preproject PRS ratings. This was not unexpected, since the ARS ratings were obtained at the close of the experimental period. Note that the

correlation between the preproject and the postproject PRS scores was not much higher than the correlations between the preproject scores and the attendants' ratings. The preproject-postproject PRS correlation was lower than the correlations between the postproject PRS scores and the attendants' ratings. The intercorrelations among the three ARS items were of the same order of magnitude as their correlations with the postproject PRS scores. These results suggest that the lower correlation of the postproject PRS scores with the preproject PRS scores reflected a genuine change in the relative statuses of patients.

The correlation between the two PRS scores is interpreted as a minimum PRS reliability estimate, since a different psychiatrist rated each patient at the conclusion of the study, without knowledge of the patient's preproject rating. Similarly, because the attendants completed each item of the ARS without opportunity for comparison with each other's ratings or with their own ratings on other items, the intercorrelations among the item ratings are interpreted as estimates of the reliability of the ARS.

The reliability of the measures used in this study does not establish their validity. It is conceivable that neither the PRS nor the ARS was valid and that their mutual agreement merely reflected the operation of some common bias. However, this seems highly unlikely. The measurements were made independently in different ways by people with highly disparate professional backgrounds. There does not appear to have been sufficient similarity between the two rating procedures to provide a basis for a common bias. The only obvious common factor was the patients' clinical statuses. Moreover, since the PRS raters were not personally involved in the study, there is no reason to believe that they were motivated to find improvement. Therefore, the PRS is considered the validating criterion for the ARS.

The correlations reported in Table 2 are probably underestimates, because the patient group was screened. If the patient sample had been more heterogeneous, more like a typical back ward group (including a scattering of senile or organic "incurables"), then the correlations would almost certainly have been even higher.

The ten ward attendants' consentient evaluations of their patients agreed with and were as reliable as the professional evaluations by the psychiatrists although the attendants did not have access to the PRS ratings of the patients. This suggests that it would be feasible to use attendants to make evaluations heretofore made by professional personnel only. This is not to imply that it is superfluous or unnecessary to have psychiatrists evaluate patients. Rather, the implication here is that competent attendants' impressions of their patients may be a valuable adjunct to psychiatric ratings. Psychiatrists cannot be expected to complete a PRS rating of their ward patients every week. But attendants may certainly be asked to make routine evaluations of their patients' statuses. Any significant changes in a patient suggested by

the attendants' ratings might then be brought to the attention of the psychiatrist responsible for the ward. He could then take appropriate action, which might include professional evaluation of the patient.

Moreover, since there were individual idiosyncratic perceptions by attendants, attendants' ratings of their patients may prove useful for uncovering peculiar perceptions by particular ward aides. If an individual attendant persistently disagreed with the consentient rating of particular patients, this might indicate a need for further examination of this attendant's work and personal relations with those patients.

Conceivably, attendants might reach a group consensus evaluation of their patients if a rating procedure were maintained routinely. In this study, the ratings were taken only once, and the attendants were not informed in advance of the plan to have them rate the patients. However, it should be recalled that the attendants who rated the patients had participated in the milieu therapy program with its group discussion sessions for attendants and other staff. In these meetings, there was ample discussion of the various patients and much more opportunity for attendants to arrive at a group consensus evaluation than is normally likely. This is particularly true for attendants who work on different shifts since they ordinarily do not have much contact with one another. The findings of this study seem to justify a long term experimental program of attendant evaluation of patients to determine whether attendants would continue to make independent ratings.

SUMMARY

The present investigation is part of a larger research project evaluating six months of milieu therapy of 61 chronic regressed female schizophrenics on a special experimental ward. At the end of the six month project, the ten attendants assigned to the experimental ward rated each patient, using a three item rating scale (the ARS) designed to uncover attendant subjective attitudes about individual patients. These ratings were compared with the Psychiatric Rating Scale (PRS) evaluations of the same patients obtained at both the start and the end of the project.

The grand mean of the coded ARS ratings was significantly more favorable than the neutral point of the ARS scale. Item 1, pertaining to time monopolization by the patient, elicited less favorable ratings than did the other two items, pertaining to how suitable the patient was for milieu therapy and how pleasant the patient was to work with. Analysis of variance of the ARS ratings revealed that not only the scale as a whole but also each individual item discriminated among patients (ratees) and among attendants (raters). However, significant interactions among the variables indicated that each item reflected different aspects of the relationships between patients and attendants. There were high intercorrelations among the ratings elicited by the different ARS

items, and the ratings elicited by each item correlated highly with the PRS ratings obtained at the end of the project. It appeared that the pooled ARS ratings were as reliable indices of patient status as the PRS evaluations. Attendant evaluations may provide useful ongoing measures of their patients' statuses, freeing professional personnel for less routine tasks.

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HOW TO MAKE AN IMMIE HOLE

JOSEPH LYONS

Veterans Administration Hospital, Lexington, Kentucky

The game of immies, now commonly called marbles and known for centuries as ring taw, is the only competitive sport in which a small boy may win a world championship. The rules by which this is established are wondrously elaborate. They provide that the marbles—that is, the mibs, miggs, commies, hoodles, or ducks—shall be between one-half and five-eighths of an inch in diameter, and that the shooters—also known as taws, moonies, or glassies—shall be between five-eighths and three-quarters of an inch. A row of thirteen marbles is placed on a cross in the center of a ring which is ten feet in diameter. One marble is placed at the point of intersection of the arms, and three are distributed on each arm at three inch intervals. The object of the game is to knock the marbles out of the ring with one's shooter but without leaving the shooter in the ring. Forbidden are such moves as histing, or lifting the knuckles off the ground before the shooter leaves the hand, and hunching, or moving the hand forward before shooting. In tournament play all games are for fair rather than for keeps, and marbles are returned to their owners after each game (Forbush and Allen, 1954).

There is probably not an adult in the Western world who is not familiar with this game, hardly a man who has not played it in some form as a boy. Of course, the conventions and practices which govern informal play in the neighborhood are different in many respects from those outlined above. Informal rules, for example, are usually not spelled out precisely and in any case are rarely formulated publicly, as are those for tournament play. The latter are published in rule books, the former apparently adjusted to the changing circumstances of the day, the mood of the players, or the lay of the land. Neighborhood games are attuned to the hazards of the moment, whereas in tournaments untoward events are controlled in advance by proscription. Therefore official rules call for a referee, who is a phenomenon peculiar to organized activities. His function is to supervise and thus to eliminate the effect of events which cannot be subsumed neatly under a set of rules. In the informal game if his shooter slips from a boy's hand he is entitled, by a tacit but universally binding rule, to cry "Slips!" If he has spoken soon enough, he picks up the shooter and tries again, without penalty. In tournament play, however, the referee must in the inscrutable way of his kind decide that it was really a slip before he will approve it. The role of the referee, as independent arbiter and exegetical authority in regard to the rules, sharply distinguishes formal and organized games from those unplanned and informal.[1]

[1] It is interesting to note, in this connection, that in professional wrestling, which has lately become less sport than spectacle, the referee alternates between the roles of stern arbiter and clownish participant.

These issues have to do with that most weighty of matters, the nature of the rules by which men--and boys, too--attempt to govern their conduct. In this essay I will explore some of the differences between formal and informal rules, particularly as they apply to the game of immies. Now, there are in general two ways that one might undertake this sort of examination. The first is familiar to us as students and as practitioners. It consists in studying the problem from the outside, as it were, by taking up a position within one of the admirable disciplines concerned with games--sociology, child psychology, anthropology, perhaps psychoanalysis. The second method is less well known and in fact not easily presented except by precept. The following exemplary passage by the Quaker essayist Logan Pearsall Smith, though it has to do with statistics rather than with rules, will suggest what I mean:

"When I read in the *Times* about India and all its problems and populations; when I look at the letters in large type of important personages, and find myself face to face with the Question, Movements, and great Activities of the Age, 'Where do I come in?' I ask uneasily . . . Then in the great *Times*-reflected world I find the corner where I play my humble but necessary part. For I am one of the unpraised, unrewarded millions without whom Statistics would be a bankrupt science. It is we who are born, who marry, who die, in constant ratios; who regularly lose so many umbrellas, post just so many unaddressed letters every year. And there are enthusiasts among us, Heroes who, without the least thought of their own convenience, allow omnibuses to run over them, or throw themselves, greatheartedly, month by month, in fixed numbers, from London bridges" (1945, pp. 52-53).

Here is the viewpoint I will be advocating. As you can see from the example above, it consists in seeing the problem from the inside out; the writer peering out at his reader through ordinal slits in the normal curve. Applied to the matter of rules, such an approach would begin by investigating, not the history or conditions or logic of making rules and abiding by them, but how it seems to the person as he goes about doing this. If we are persistent and a little lucky, we may from this beginning be led to some useful generalizations concerning the meaning of rules in that world of action we all share as human beings.

We turn, then, to the players themselves, who are most directly involved in the game of immies. Fortunately, their particular view of the matter has recently been stated by another thinker named Smith. Though he chooses to disguise the profundity of his insight by a mock serious tone, his message is clear. This is what he has to say about the rules which determine the opening of a game:

"The rules were that as soon as the ground got over being frozen, any right-minded kid on his way home from school, or in recess, planted his left heel in the ground at an angle of forty-five degrees and walked

around it with his right foot until there was a hole of a certain size. You couldn't measure this hole. We all knew what size the hole was supposed to be. I could go outside right now and make a hole the right size. (I did. It's still the same size. The size of an immie hole. And while I was outside I drew a line with the toe of my foot the proper distance from the hole. It's still the same distance. It isn't something you measure in feet. It's the distance from the immie hole that the line is supposed to be)" (R. P. Smith, 1959, pp. 4 and 7).

Although the method of tackling a problem from the inside of it seems at first glance to give rise to an unfamiliar kind of scientific proposition, there is at least one fundamental respect in which Smith's statements are unobjectionable. He insists that the immie hole as he constructs it is measurable. He would, I think, agree with Stevens that "when description gives way to measurement, calculation replaces debate" (1951, p. 1). Smith maintains that the true immie hole is of a definite size, that this size is measurable by a trained or experienced observer, and most important, that for him the canon of replicability of measurement must hold. "I could go outside right now and make a hole the right size," he states. "I did. It's still the same size." Tested by an operational strategy, as it should be, the size of his immie hole is shown to be a quantifiable entity.

Wherein lies the difference, then, between the Smith approach and that of the more conventional scientist? It is clear that it must reside in the *unit* of measurement. The proper immie hole, according to Smith, is of a size which "isn't something you measure in feet." The "right size" of an immie hole, he maintains, is "the same size." If we now ask, "The same size as what?" his reply will of course be: "The size of an immie hole."

Now that we are quite clear on the logic of Smith's position, we are prepared to examine it in depth. The first objection we must consider is that he appears to be arguing in a circle. He defines a whole in terms of its constituent parts and concludes the argument triumphantly by defining the part with reference to the completed whole. He pulls himself up by his own bootstraps—and then insults our intelligence by patting himself on the back while holding the bootstraps and meanwhile thumbing his nose at us with his free hand. His is a method which begins and ends inside the problem it undertakes to explicate, and therefore it always seems to be going round in a circle. But since whatever merit the method has is revealed in this result, we will take his argument seriously, as it deserves, and trace out the consequences.

There is another measurement involved here, which Smith characterizes only by the phrase "the proper distance." It refers to the distance of the line from the immie hole. Since each player is required to stay behind the line to shoot toward the hole, the outcome of a game is determined largely by the distance of the one from the other. Yet

in the Smith system of measurement this quantity is never stated exactly. This is not because Smith does not know it—on the contrary, he recognizes it immediately as correct—but because he does not possess it in the form of an amount, expressed in figures, which he knows in advance of making the hole or drawing the line. It lacks that characteristic of the publicly stated rule, which is that we know it independently of any or all games. Rather, this critical distance seems to be discovered each time that preparations are made for playing a specific game. It comes into being, so to speak, as a natural first step in playing the game itself. The distinctive character of a rule is that it has existed before the game began, will persist after the game is finished, and would remain the same rule even if no small boys ever played marbles again. The Smith distance, on the other hand, though it may appear to be a determining condition of the game in general, actually has more the quality of the playing of one game in particular. Therefore the precise distance of line to hole can no more be abstracted from a particular game than could the moment-to-moment playing. Both belong in one certain game, gain their significance from it, and have no life outside of it.

There is a serious-minded little boy (named Brown rather than Smith) in a currently popular comic strip who not long ago was shown over the course of a week in the throes of reliving a particularly traumatic experience: he had let the third strike go by without even attempting to swing at it. Each night he would awake in a cold sweat, caught in the agony of that moment remembered, and cry, "I could at least have swung at it!" Thus we learn that in memory the emotion changes utterly; here regret follows on what was initially free of regret, on what may even have been regret's opposite. Similarly, the criminal is never guilty *as* he commits his crime, only afterward. My point, and Smith's too, is that memory provides one instance of being removed from an initiating situation, and that this is why the later emotion can be so changed and so much more or less intense. By contrast, the critical distance of the Smith measurement can never be separated as though it were a thing remembered. It is a distance which is found only within a specific game, and in fact only by playing the particular game or participating in it. But, though the distance can never be defined as such before a game begins, no participant has any difficulty stating it once he begins to set heel in dirt—provided, that is, that some other absolute rules are also observed, for example, that the ground is sufficiently over being frozen. The locus of this distance, then, must be at the very inwardly spiralling center of the particular, concrete experience of beginning a game. If we tried to track it down, we might be sucked further and further through it, finally perhaps to disappear down the throat of our own action.

But I digress from the serious issues at hand. One all-important consequence of discovering the relevant aspects of a game entirely by

participating in it is that what goes on is taken seriously. Thus the spirits which govern the activities of the small boy have decreed, and quite properly, that when he is in the society of his peers he will consider it disgraceful to play without playing for keeps or to cry if he loses some of his marbles. With prodigious righteousness he will bear witness to the truth that any other sort of behavior belongs to girls or to such infra-human follies as birthday parties. It is for this reason that child's play, though it may sometimes be easy, is never frivolous. Children, like other leaders of men, live hard, and by God, they play hard. Immies is for keeps.

The contrast with organized games, however, is so striking that it can hardly be accidental. Tournament play, though it is rigorously organized, precisely conceived, and hemmed in by rules which permit no deviation, is always "for fair." It is like amateur sport in general, of which its devotees say that what counts is not whether you win but how you play the game. Starting with this unreal conception, which perfectly describes an act with no consequences, it is quite understandable that amateurs have always needed some extraneous value to attach to their pursuits and provide the facsimile of a goal. Pride in one's alma mater serves the purpose for the more innocent of our high school athletes; and wealthy sportsmen are often satisfied by indulging a need not open to everyone, to excel in a sport which is restricted to their class. But without such frostings, play for its own sake is hardly more than play. He who indulges in it, if he attains some reflective understanding of his own state, will discover that he is only dribbling sand endlessly between his fingers.

Is this merely a chance dichotomy, or is there an intrinsic relation between the seriousness, the informal organization, and the concrete participation which marks neighborhood games, on the one hand, and the goal-lessness, the formal organization, and the independent character of its rules, all of which define the tournament game? It is the boy who chooses to play *with* marbles for keeps, and the adult who arranges for him to play *at* marbles for fair. The distinction, then, is between the child, whose play is almost a matter of life and death, and the grownup, whose action is often an attempt to evade such finality. Here is our clue. The rules and the measurements by which small boys abide are final and absolute. That this occurs as a spontaneous and natural by-product of the random individuality of each player is a profound paradox meriting serious attention by philosophers of science—but the simple fact is that the measurements which bind the actions of small boys in a casual game of marbles informally arranged carry the awful and impenetrable authority of dicta from on high. The "proper distance," to use Smith's valuative terminology, between shooting line and immie hole is not an approximation to some ideal condition but the flawlessly exact space itself as realized in the act of shooting. Contemplate this as you recall that the mark of every proposition in science

is its probabilistic nature. All its measurements are inexact, all its acts merely approximations to an ideal and therefore unrealizable state. The strongest of scientific constructions are at their basis arbitrary and at their best only probable. Nothing but a widespread and progressively delicate net of interrelated propositions, taken all together, lends the weight of successive approximation to each principle in science—but the structure is this finely articulated, that the discovery of one instance clearly contrary to the others suffices to bring it all crashing down in a heap, on top of a dusty pile of manuscripts about phlogiston and black bile. As much as the laws of a boy's game of marbles are grounded solidly on unalterable certitude, so much does the structure of his father's science rest precariously on a sort of chaos, lighted only by an odd and unrecognized faith.

In the tournament, and of course in other adult activity as well, and particularly in science, its epitome, rules as well as measurements form a set of more or less arbitrary conventions. They work; that is the very most that can be said for them. They enable adults to get on with their doings as they busily, mysteriously, hurry toward ends which make little sense to small boys. Thus all the measurements which adults make have this in common, that they require some accepted ruler, and an order issuing therefrom, toward which the person genuflects, at least implicitly, each time that he calculates the distance between two points. A bar of platinum-iridium in a vault at Sèvres, near Paris, is the best known of these standards, but it is only one example out of many. Notice, too, that these rulers form a kind of coalition. They stand or fall together. If it turns out that one is untrustworthy, all become suspect, and therefore if one is cast out, all must abdicate.

But where in all the world is situated the metric bar by which one may measure the "proper distance" that Smith describes, the distance from the immie hole that the line is supposed to be? There is an alternative possibility, fearsome though it may seem to us as sober scientists: that the location of *this* bar is in a boy's soul. It must then follow as a natural consequence that the day a boy refuses to seek there for the absolute rule by which to measure an immie hole, that day he is on his way to becoming an adult, perhaps even a scientist. It is the day on which he begins to doubt all absolute measures. He suspects, and rightly, that standards of measurement are never more than copies of the Paris original. He becomes aware of how uncertain and variable is the performance of any of them as rulers. He may even come to learn, Lord help him, that the final ruler in Paris has its own weaknesses and imperfections. In this way he will come to grasp, this poor, miserable, precise, ordered creature, that no measure at all is ever exact, that all truths are approximate only, that he is surrounded by rulers yet resident in anarchy. He will finally have traded his soul for measurement and chaos.

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THE EFFECT OF A PULSATING MAGNETIC FIELD UPON THE BEHAVIOR OF MICE

WILLARD NELSON AND MELVIN NEELEY
Florida State University

Results of previous experiments indicate that an electromagnetic field stimulates sensation in the peripheral area of the human retina (Barlow, 1947; Thompson, 1951). Ultra-high frequency electromagnetic waves are also said to bring about functional changes in the nervous system (Livshits, 1947). When a conductor of electricity is subjected to a fluctuating magnetic field, a current is induced in that conductor. The choroid plexus, which manufactures spinal fluid, is said to contain a membrane which conducts electricity like a wire (Steihler, 1938).

Although Yeagley (1947) found some support for his theory that pigeons use the earth's magnetic field in homing, this theory is not now widely accepted. For example, Orgel and Smith (1956) reported that pigeons are insensitive to a 5 gauss magnetic field (approximately 25 times that of the earth). The problem of sensitivity to magnetic fields does not appear to have been extensively investigated in common research animals such as the rat and mouse.

The purpose of the present experiment is to determine whether significant changes in behavior occur when mice are subjected to a strong pulsating magnetic field. Since a strong magnetic field induces more current in a conductor which cuts that field than does a weaker field, it was assumed that a stronger field would be more likely to produce an effect in an animal. Mice were chosen for subjects in this experiment because of their small size relative to the obtainable concentration of field.

METHOD

Subjects and Apparatus

The Ss were 14 white mice of the Swiss Webster strain. Hunger motivated the Ss to seek food.

The apparatus was a modification of that used by Maier in his reasoning experiments. Runways 120° apart radiated from a circular center platform 12 in. in diameter. The runways were 2 in. wide and 24 in. long. Each runway had a smaller platform at its distal end where the goal object was hidden from view behind a small screen.

Three electromagnets were placed beneath the points at which the runways joined the center platform. The magnet beneath the end of the runway leading to the goal on a particular trial was always energized and the other two magnets were neutral. As measured at a point 1 in.

above the platform, the strength of the magnetic field was 3600 gauss or 18000 times the earth's magnetic field.

Procedure

The mice were placed, one at a time, upon the midpoint of the center platform. The goal object was a food reward consisting of 1½ cc of cheese. The goal could be reached by random searching, in which case the correct runway would be chosen initially by chance probability once in each three trials. Any significant variation from chance in the selection of the correct runway would indicate sensitivity to the magnetic field and the association of the field with the food.

The goal object and the corresponding magnetic field were alternated randomly from one of the runways to another to prevent positional cues. Small pieces of the cheese were scattered in a circle around the apparatus to prevent olfactory cues. In order to counteract the remote possibility that the small amount of heat generated by the magnets would give cues, the center platform was rotated and no one magnet was used more than a few minutes at a time.

Seventy-five runs were made using the magnetic field alone as a cue to the runway containing the food. Then shocking grids were placed at the entrance to each runway. Twenty-five runs were made with shock on the incorrect runways. Ten runs were subsequently made with the grids left in place but with no shock. Twenty final runs were made with the grids removed as in the first 75 runs.

RESULTS AND DISCUSSION

During the first 75 runs none of the Ss chose the correct runway more often than chance probability. During the sequence of shock trials some of the Ss became disorganized and refused to move from the center of the platform. Three of the Ss continued to run but did not choose the correct runway more often than chance. But, during these 25 runs, 5 of the Ss selected the correct runway as their initial choice 97 times in 125 subject-runs. This variation from chance probability is significant at the 1% level.

In order to determine what cue they were using in choosing the correct runway, only these 5 Ss were used in the remaining runs. When the shock was discontinued but the grids were left in place for an additional 10 runs, these 5 Ss continued to choose the correct runway significantly more often than at a chance level.

When the electric grids were removed from the apparatus and 20 more runs were made with only the magnetic field as a cue, the 5 Ss reverted to random searching, failing to select the correct runway as their initial choice more often than chance.

Even though an attempt was made to use nonmagnetic material in the shocking grids, 5 of the Ss evidently perceived either a slight vibra-

tion or a slight current caused by the fluctuating magnetic field beneath the grids. This secondary cue was not strong enough for the experimenters to perceive it, yet it is evident that the cue used by the 5 Ss had to do with the grids' relationship to the magnetic field rather than to the direct effect of the field. From these results it appears that magnetic fields approximating 3600 gauss and 120 fluctuations per sec. do not produce an adequate stimulus in white mice.

SUMMARY AND CONCLUSION

The purpose of the study was to determine whether animals might be stimulated by a pulsating magnetic field and could then use this stimulus as an identifier of the shortest path to a goal. White mice were used as subjects because of their small size in relationship to the obtainable concentration of field. The conclusion was that a pulsating magnetic force approximating 18000 times the earth's magnetic field and 120 fluctuations per second does not affect the behavior of mice in this experimental situation.

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JUDGMENT OF FAMILY RESEMBLANCE AS A FUNCTION OF CONSANGUINITY AND SOCIAL EXPECTATION

G. Y. KENYON AND HENRY WINTHROP[1]
University of Wichita

The problem to which this paper is addressed is the following: In social intercourse, when subjects are truly told that a given adult male and a specific boy are father and son and thereafter note a resemblance between the two, is the alleged resemblance a fact of structural similarity, pure and simple, or is it the product of suggestion operating through social expectation? In other words, if there were some *objective* method for determining degree of resemblance, how often would we find that *declared resemblance* is a socially patterned expectation rather than a fact of consanguinity? The experiment reported here represents one method for determining the effects upon social judgment of family resemblance of a variation in and cross-play between two factors. These are, the presence or absence of actual consanguinity and the furnishing of information that family relationship exists or is absent, where this latter information is sometimes true and sometimes false.[2]

METHOD

Materials employed. The stimulus materials used in the present experiment were the 16 pairs of photographs of fathers and sons, taken from *Life Magazine* (Oct. 15, 1956) and mentioned in footnote [2] below.

Procedure. A 2 x 2 factorial design was employed in order to alternate adult-boy relationships with social expectation. Four groups consisting of 20 Ss each were exposed to the following experimental conditions. G_1 was shown the 16 consanguineous (C) pairs and the examiner *declared* that these were related (D_C). G_2 was shown a fixed pattern of 16 pairs of photographs in which the adult-boy members were not at all related (C′) but which were likewise declared to be related (D_C). G_3 was shown the consanguineous arrangement (C) which was declared to consist of unrelated pairs ($D_{C'}$). G_4 was shown the fixed non-consanguineous arrangement (C′) which was declared to consist of unrelated

[1] Now at the University of South Florida, Tampa, Florida

[2] The interest in this question has the following history. In 1956 a Dallas clothing store conducted a "chip off the old block" contest wherein free clothing was awarded to father and son pairs who most resembled each other in the eyes of the judges. Out of the 260 pairs competing for clothes *Life* Magazine published the pictures of the three winners along with 13 other father and son pairs who entered the contest. Preliminary matching trials by 13 subjects (with knowledge that the pairs were related) were executed on the assumption that those photographs of fathers and sons would be matched correctly in frequency according to the similarity of the features of the pairs. The order of frequency of the top three was not in agreement with the clothing contest judges. The trial matching technique indicated that the first prize should have gone to the second prize winner and the winners of the first and third prizes were out of the running and that second and third prizes should have gone to two of the losers. This discrepancy suggests that variables such as gestures, language, dress, etc. may have influenced the decisions as would facial expression and neck attire in the paired comparison findings. The published photographs frequently had fathers and sons both wearing bow ties, or open neck shirts, smiling, etc. in common.

adult males and boys (D_C'). The layout of the photos in all cases consisted of two horizontal rows set before the Ss, the upper row containing adult males and the lower row of boys.

The context in which the "atmospheres" were created involved four different sets of conditions which were read to the Ss who, at the same time, also read the conditions on their instruction and answer sheets for themselves.

G_1(C x D_C). The pattern of actual, paired relationships (C) was presented to this group with the following instructions (D_C):

> You are being presented with 16 pairs of photographs. Each pair consists of a natural (i.e., blood relation) father and son. Each matched pair has been coded for purposes of identification, the father in each pair being given a number, the son a letter. You are being asked to examine these 16 matches carefully, one at a time. If you think the members of any pair facially *resemble* each other, then you are to place the letter "A" meaning "they look *alike*" in the answer box just below the matched pair as coded below. If you think they *fail to resemble* each other, place a "U" meaning "they look *unlike* each other" in the answer box.

Following these directions an answer box was provided on the instruction sheet, consisting of two rows. In the upper row the S placed his A or U. In the lower row he inserted a scale number which expressed his degree of confidence in his judgment. This was done in terms of a 6 point graphic rating scale running from +3 to −3 (certain to very uncertain). These latter intensity ratings are not analyzed in this report.

G_2(C′ x D_C). The fixed C′ pattern was presented to this group, rather than the C pattern, but the instructions were also D_C.

G_3(C x D_C,). For this group the C pattern was provided with the D_C' instructions which are presented at this point.

> You are being presented with 16 pairs of photographs. Each pair is a *random* match or combination of two males, one an adult and one a boy. In other words, the members of each pair are *completely unrelated* to each other. Adoption agencies frequently encourage adoptions, assuming that all other considerations have been met, when a boy appears to resemble his *prospective* father by adoption. We are asking you to look over very carefully the 16 random matches in front of you. If you were the official of an adoption agency, responsible for encouraging adoptions where there appeared to be a resemblance between *prospective father* and *prospective son* you might have to exercise judgment on these 16 pairs. In your judgment which of

these pairs involves males who *resemble* each other? If you think that the members of any pair facially resemble each other, then you are to place the letter "A" meaning "they look *alike*" in the answer box just below the matched pair as coded below. If you think they fail to resemble each other, place a "U" meaning "they look *unlike* each other" in the answer box.

These $D_{C'}$ instructions were followed as usual by the C pattern, then the answer box and finally the confidence scale.

G_4 (C' x $D_{C'}$). For this group the C' pattern and the $D_{C'}$ instructions were provided. Both of these have been described above.

No effort has been made in the analysis which follows to correlate the median scale values of the confidence scale furnished by our subjects, with the actual, empirical accuracy of their judgments in situation C. Other data were also solicited from the subjects at the conclusion of this experiment and were recorded but the determination of any relationship between the judgments rendered by our subjects and these other data will have to be reserved for separate publication.

RESULTS

Homogeneity of variance was confirmed by Bartlett's test so the appropriate analysis of variance was undertaken and the summary of our results is set forth in Table 1.

TABLE 1

SUMMARY ANALYSIS OF VARIANCE OF THE JUDGMENT SCORES CONCERNING RESEMBLANCE BETWEEN ADULT MALES AND ASSOCIATED BOYS FOR 4 GROUPS OF SUBJECTS

Source of Variation	Sum of Squares	*df*	Mean Square	*F*	*P*
Nature of the relationship (C + C′)	23.113	1	23.113	4.806	$< .05$
Nature of the information furnished (D_C + $D_{C'}$)	165.313	1	165.313	34.376	$< .01$
Interaction (C + C′) x (D_C + $D_{C'}$)	15.311	1	15.311	3.184	$> .05$
Within groups	365.450	76	4.809		
Total	569.187	79			

ΣX (i.e., alike-judgment scores) for the four groups are likewise set forth at this point:

Group		
1	C x D_C	- 242
2	C′ x D_C	- 203
3	C x $D_{C'}$	- 167
4	C′ x $D_{C'}$	- 163

DISCUSSION

Combining the significant F ratio of row 1 of Table 1 with the differences in ΣX, where $\Sigma X = 409$ for C and 366 for C′, it is clear that when the members of photo pairs are actually related the Ss *judge* more of them to look alike than when in fact they are unrelated, and this will be true regardless of the information given to the Ss. We can therefore conclude that actual consanguinity is associated with a proclivity to note and declare facial resemblances. Combining the significant F ratio of row 2 of Table 1 with the differences in ΣX, where $\Sigma X = 445$ for D_C and 330 for $D_{C'}$, it is clear that when information is furnished to Ss that members of photo pairs are related, these members are judged to resemble each other more often than pairs which are declared to be unrelated, and this will be true regardless of whether or not consanguinity exists. We can therefore conclude that it is likewise true that *information*, whether spurious or not, that subjects are related, is associated with a proclivity to note and declare facial resemblances on the basis of social expectancy. The absence of a significant interaction effect is touched upon in the discussion which follows.

It is to be noted that in this experiment *judgments of resemblance only* under varying conditions are being compared but *not* the accuracy of judgments under those conditions, although for G_1 and G_3 *judgments of resemblance* are, at the same time, also *accurate judgments of true relationship.*

Thus the answer to the original question posed by this paper is that both factors studied here influence judgments of presumed facial resemblance between the photographs of adult males and associated boys. The effects of actual consanguinity are reinforced to some degree by the information that the photo pairs are fathers and sons ($C \times D_C$). However for convenience in interpreting the trends in the interaction terms the following table showing the relative contributions of the strengths of relationship on instructions is offered:

TABLE 2

DIFFERENCE SCORES (ΣX) WHEN ONE FACTOR IS VARIED AND THE OTHER HELD CONSTANT

Groups	Factor held constant	Factor varied	Difference due to variation in varied factor
1 & 3	C	D_C & $D_{C'}$	75
1 & 2	D_C	C & C′	39
2 & 4	C′	D_C & $D_{C'}$	40
3 & 4	$D_{C'}$	C & C′	4

Table 2 indicates that if relationship is fixed (C or C′) then the differences due to varying instructions are 75 and 40. If instructions are fixed (D_C or $D_{C'}$) then the differences due to varying relationship

are 39 and 4. Taking the pairs of differences in the two cases suggests a more potent influence for instructions than for relationship. This interpretation is reinforced by comparing groups 2 and 3, i.e., when positive instructions accompany group 2 for unrelated photo pairs, ΣX for judgment "alike" is greater (36) than when, in group 3, negative instructions accompany the fact of actual consanguinity. In terms of statistical considerations, however, the significant F ratios indicate that judgments of "alike" will be determined by either actual consanguinity (C) or declared consanguinity (D_C), that is, by actual, objective family relationships or by expected resemblances between alleged father and son pairs.

One should not lose sight of the fact that in this study only judgments of resemblance made by our *Ss* are being studied under variable conditions and *not the accuracy of such judgments.* This is true even though such judgments are, in fact, empirically valid under condition (C). There is a distinction between a judgment that a *presented* pair of photographs show facial resemblance and the task of pairing off actually related pairs of fathers and sons, based upon alleged resemblance, given n such possible pairs of photographs. The former task involves chiefly psychological processes of recognition or, in dubious cases, judgments of marked, weak or non-existent similarity. The latter task involves fundamentally a comparison of any given photograph with n-1 other photographs, that is, the implicit construction of a scale of similarity by the method of paired comparisons. Both types of task, of course, are influenced by secondary cues of facial expression, dress, etc.

On the basis then that the mean number of judgments of "A" would equal 8 *by chance alone,* the question arises as to whether the obtained mean number of judgments in each of two of our experimental conditions, namely, ($C \times D_C$) and ($C \times D_{C'}$), is significantly different from a chance score. On the assumption that probabilities obtainable from the normal curve are fairly good approximations of the probabilities which would be obtainable from the binomial expansion, and given a mean of 8.00 and a sigma of 2.00, only the Z score of 2.0475, corresponding to the mean of 12.10 for the experimental condition, ($C \times D_C$), yields a value for $p<.05$. To be exact $p=.0203$. We can therefore conclude that *judgments* of "A" in G_1 represent a significant departure from chance or, to put the matter another way, judgments that resemblance exists between pairs of adult males and boys, based upon photos, are likely to be accurate only where the paired photographs *actually represent* father and son relationships and are, in addition, accompanied by the information that the members of these pairs *are actually related.*

SUMMARY

An experiment was undertaken to see what the effects of actual consanguinity and social expectation are, in judgments of the degree of resemblance between selected pairs of photos of adult males and boys.

Conditions were varied so that these pairs of photos sometimes represented actual consanguinity and at other times represented fictitious consanguinity under two sets of contradictory instructions concerning the relationships. The findings indicate that both consanguinity and social expectation affect judgments of facial resemblance, with social expectation playing the greater role.

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RELIABILITY OF CONTENT-FREE, GROUP VERBAL BEHAVIOR[1]

EDWIN O. TIMMONS, HENRY C. RICKARD, and ROBERT E. TAYLOR[2]

V. A. Hospital, Tuscaloosa, Alabama

A major goal of this research is the development of a reliable means by which changes in patients' verbalizations during group psychotherapy are measured. Elements of S's verbalizations may be thought of as dependent variables, which can be assessed before, during, and after the experimental treatment. Before verbal measures may be employed as criteria, however, their stability must be demonstrated. We hypothesized that a group of Ss would make similar responses to different interviewers if the Es would make themselves similar verbal stimuli by following interview rules. The rules specify duration and, to a large degree, frequency and direction of the E's remarks, but they allow almost complete freedom in terms of content. We have devised modifications of the Saslow and Mattarazzo (1958) individual interview for use with groups.

A first experiment and two replications are reported here. All utilize the same procedure and the same population of Ss. The combined results of all three studies are also examined.

PROCEDURE

Experiment I

A group of 5 chronic neuropsychiatric patients were interviewed by two Es separately for a total of three 50 minute sessions separated by three day intervals. Patients in this and the following experiments were selected by their ward physician, who was instructed to choose Ss who were non-organic, under 50, and who would make at least minimal verbal responses when addressed. All Ss were unknown to the Es. E1 interviewed the group on the first and third days for the first 25 minutes. He was interrupted by E2 who became the interviewer for the next 25 minutes.[3] E2 interviewed the group the first 25 minutes of the second session and was followed by E1. Before the first session, E1 explained to the group that the interviews were being held to learn more about patients on that ward, and that they had been chosen as representative individuals.

Directions. The standardized rules used by the Es were as follows:

[1] We wish to acknowledge the participation in this study of all members of the Tuscaloosa V. A. Hospital psychology staff, especially Dr. John Kew, Frank Horner, and Robert Ilardi. This project could not have been accomplished without the cooperation of Mildred J. Stingley, M.D., Wiley D. Lewis, M.D., and William B. Robinson, M.D., who furnished subjects. Dr. Robinson also served as control interviewer in Experiment III. We owe much to the related experimental work of Matarazzo and his associates. Lastly, we are indebted to Dr. Paul Seigel and Dr. W. O. Jenkins for careful readings of the manuscript.

[2] Now at the University of Tennessee.

[3] H. C. Rickard was E1 while E. O. Timmons served as E2.

Elicitation occurs during the first, third, and fifth 5-minute segments of the interview. The second and fourth segments are stress periods. Elicitation is performed as follows:

E begins the session by signaling the observers; he then swings his gaze around the room to allow 10 seconds for spontaneous S comments. If no S makes a remark, the *E* makes a 5 second general comment. He waits 5 more seconds, and if no reply is forthcoming, *E* picks a S at random and directs his gaze toward him. If the S does not speak within 5 seconds, *E* directs a 5 second comment to the subject. In the event the S replies to *E*'s remarks, the interviewer directs another comment to the S; if the S replies again, *E* directs a third comment to the S. This procedure allows a total of three remarks by the patient, and shall be referred to as a "round" of interaction. During such an interaction the *E* never waits more than 3 seconds before responding to an S comment. After S replies for the third time *E* averts his gaze and waits 10 seconds before beginning the same sort of interaction with the next S. If during that 10 second waiting period the S immediately addressed speaks again to the *E*, the interviewer replies for another full round of interaction. If *E* has difficulty cutting off S after the second round of verbal exchanges, it is permissible to direct an immediate comment to the next S without waiting the usual 10 seconds. After each S has been addressed in a random order, the interviewer again begins the cycle of elicitation. If during a silence or during elicitation from a given S, another S addresses *E*, the interviewer must reply to the intruder and elicit from him a total of three comments. After this interaction the *E* completes his elicitation round with the S who was being addressed prior to the interruption. *E* makes all of his comments as non-directive as possible, but he is permitted to ask questions. The *E* does allow "S to S" verbal interaction which occurs at any time during the interview.

During the stress periods the *E* refrains from replying to a comment or addressing a patient. He occupies himself with note-taking and does not look at any S. Even direct questions cannot be answered by the *E* until the beginning of the next elicitation period.

Each *E* held a clipboard in his lap with a stopwatch affixed to the top. It proved helpful, in equalizing elicitation, to make a check mark by the name of a S whenever a verbal exchange was initiated.

It can be seen that the rules deal with the frequency, duration and direction of *E* verbalizations rather than with content. For example, the *E* attempted to make each of his comments run about 5 seconds in length. An effort was also made to elicit verbal behavior an equal number of times from each *S;* since Ss could also initiate verbal interchanges, it was anticipated that some patients would certainly verbalize more than others. When these rules are employed, any S other than a mute individual will make at least minimal verbal contributions. In

addition, there is room for wide individual differences in verbal expression to emerge.

Prior to Experiment I, each *E* practiced the standardized group interview for ten sessions with staff psychologists and psychology trainees in a simulated group therapy situation.

Frequency of speaking and number of minutes of speech for each S were taken from tape recordings. Mean length of speech, a third measure, was derived by dividing frequency into amount of speech. Because of the difficulty in recognizing S's voices, the first session was scored simultaneously by the two *E*s, allowing an opportunity to mutually identify the Ss. The last two sessions were scored independently by the *E*s.

Experiment II

A second group of 5 neuropsychiatric patients, this time from an acute ward, were interviewed for two 25 minute sessions by *E*1, and three 25 minute sessions by *E*2 in a partially counterbalanced order. One *E* became ill before the full design could be completed. The same interview rules were followed. Since taking data from a tape recorder was found to be time consuming and difficult, a measuring apparatus was devised. Measurements were taken by an operator who depressed the points of fixed pens, one for each S, on the surface of a sheet of paper moving at a constant speed. The lines thus obtained were measured and converted into frequency data and minutes of speech per S. Since only speeches over 2 seconds in duration were counted, grunts, "uh huhs," etc. were not included. While the Operator of the apparatus kept a specific pen depressed for the duration of each S's speech, a second individual called the Recorder wrote a symbol representing the S being addressed on the line drawn by the apparatus. For purposes of establishing reliability, a second Recorder independently noted on a sheet of paper the direction of each S's speech. From this information the following direction of speech measures were derived: number of times each S was addressed, number of times each S addressed *E*, number of times each S addressed a participant other than *E*, and number of times each S was addressed by participants other than *E*. All these data were collected from an observation room equipped with a two-way mirror and speaker system.

Experiment III.

A third group of 6 neuropsychiatric patients from a continued treatment ward were interviewed for four 25-minute sessions in a counterbalanced order by *E*1, *E*2, and an interviewer (*Ec*) used as a control. The control interviewer, a ward psychiatrist who was unfamiliar with the interview rules, was instructed to view the group as a therapy situation and conduct the interview in whatever manner he chose. It was hypothesized that the Ss would respond in a similar manner to *E*1

TABLE 1
MEANS, RANGES AND COEFFICIENTS OF CORRELATION FOR *E*1 AND *E*2 IN EXP. I. THE DATA REPRESENT MEAN RESPONSES TO *E*1 AND *E*2 PER 25 MIN. SESSION

Measure	*E*1	*E*2	Tau	*P*
Minutes of Speech			1.00	.008
Mean	3.26	3.20		
Range	.67-9.50	.50-7.83		
Frequency of Speech			.60	.12
Mean	11.93	10.60		
Range	6.00-20.00	5.67-17.67		
Mean Length of Speech in Minutes			.60	.12
Mean	.23	.28		
Range	.07-.48	.07-.47		

and *E*2, but that a lower correlation in S responses would obtain between *Ec* and either of the *Es* who followed the interview rules. The same apparatus and experimental setup described in Experiment II were again used.

RESULTS

Experiment I

Reliability of Measurements. Complete agreement between the raters was found when the Ss were ranked on the frequency and duration measures taken from the tape recordings. Discrepancies were small in the scores given to the Ss by the raters; consequently, the average of the rater's scores was used as the final measure of S responses to each *E*.

Reliability of Subjects' Responses as a Function of Experimenters. Table 1 presents the length in minutes, frequency, and mean length of comments emitted by each S to the two interviewers. Scores listed are mean figures for three 25 minute sessions. The duration measure showed a perfect rank correlation between *E*1 and *E*2. The Kendall rank correlations of .60 for frequency and .60 for mean length of speech were positive, but non-significant. The Ss' responses to the *Es* were not significantly different on any of the three measures.

Experiment II.

Reliability of Measurements. Since no judgments were involved—Ss either talked or did not talk—a systematic study of the reliability of the Apparatus Operator was considered unnecessary. Determining to whom each S's comments were directed did involve a degree of judgment, however, and a reliability check against the Recorder was car-

TABLE 2
MEANS, RANGES AND COEFFICIENTS OF CORRELATION FOR *E*1 AND *E*2 IN EXP. II. THE DATA REPRESENT MEAN RESPONSES TO *E*1 AND *E*2 PER 25 MIN. SESSION

Measure	*E*1	*E*2	Tau	*P*
Minutes of Speech			1.00	.008
Mean	2.67	2.91		
Range	1.05-4.54	.74-5.13		
Frequency of Speech			.60	.12
Mean	16.93	15.70		
Range	10.33-24.67	6.50-24.50		
Mean Length of Speech			.80	.04
Mean	.13	.17		
Range	.07-.23	.11-.28		
Freq. ea. S was Addressed			.80	.04
Mean	13.90	12.80		
Range	10.00-20.30	3.50-22.50		
Freq. ea. S Addressed *E*			1.00	.008
Mean	12.40	11.00		
Range	9.30-15.30	5.00-16.50		
Freq. ea. S was Addressed by Participants other than *E*			.60	.12
Mean	3.66	5.10		
Range	1.30-6.70	.00-11.90		
Freq. ea. S Addressed Participants other than *E*			.40	.24
Mean	3.90	5.50		
Range	1.00-9.70	1.00-12.50		

ried out. A perfect correlation of 1.00 was found between Recorders on the number of times each participant, including *E*, was addressed over a 50 minute sample.

Reliability of Subjects' Responses as a Function of Examiners. Table 2 presents the means, ranges and coefficients of correlation for the two *E*s on seven measures. Duration of speech, the first measure, again yielded a perfect rank correlation. A correlation of 1.00 was also found between the number of times the Ss addressed each *E*. Correlations in terms of the mean length of speech and number of times each S was addressed were both significant beyond the .05 level. Frequency of speaking, number of times Ss addressed participants other than *E*,

TABLE 3
MEANS, RANGES AND COEFFICIENTS OF CORRELATION FOR *E*1 AND *E*2 IN EXP. III. THE DATA REPRESENT MEAN RESPONSES TO *E*1 AND *E*2 PER 25 MIN. SESSION

Measure	*E*1	*E*2	Tau	*P*
Minutes of Speech			1.00	.001
Mean	3.81	3.92		
Range	1.29-9.56	1.09-6.48		
Frequency of Speech			.60	.07
Mean	13.29	13.13		
Range	6.50-22.00	7.00-24.50		
Mean Length Speech			.80	.02
Mean	.26	.29		
Range	.19-.43	.15-.45		
Freq. ea. S was addressed			.73	.03
Mean	11.42	11.46		
Range	9.25-15.50	5.50-21.00		
Freq. ea. S addressed *E*			.20	.36
Mean	6.00	5.50		
Range	3.75-10.75	1.25-14.50		
Freq. ea. S was addressed by Participants other than *E*			.47	.14
Mean	6.29	7.83		
Range	2.25-12.00	3.25-15.50		
Freq. ea. S addressed Participants other than *E*			.67	.05
Mean	6.29	7.79		
Range	2.00-13.75	1.50-12.75		

and the number of times each *S* was addressed by participants other than *E*, yielded positive but insignificant correlations. It is notable that all seven measures are positively correlated. Examination of the ranges and mean values presented in Table 3 indicates not only that Ss' responses to the *E*s are highly correlated, but also that there is little absolute difference in the Ss' responses to the two interviewers.

Upon examining the interviewers' behavior, it was found that the average length of *E*s' comments over the sessions was 4.2 seconds for *E*1 and 5.4 seconds for *E*2. The mean number of times *E*s addressed Ss was 10.8 for *E*1 with a range from 9.0 to 13.3 and 7.6 for *E*2 with a range from 3.5 to 10.5. A rank correlation of —.30, obtained between

Es on the frequency of addressing each S, indicated that the interviewers did not show similar biases for addressing Ss.

Experiment III.

Reliability of Measurements. Again, the work of the Apparatus Operator in depressing the point of a certain pen on a moving sheet of paper while a S spoke was considered to be an objective procedure. In Experiment II a perfect rank order reliability was obtained between Recorders in determining to whom the Ss' comments were directed, and it was believed unnecessary to check reliability on the direction measures again. Three of the six Ss proved to be extremely delusional, however, and the single Recorder reported considerable difficulty in determining to whom each S's remarks were addressed.

Reliability of Subjects' Responses as a Function of Examiners. Table 3 presents the means, ranges, and coefficients of correlation for *E*1 and *E*2 on the same seven measures listed in Experiment II. The duration of speech measure yielded a perfect rank correlation as it did in both the previous experiments. Frequency of speaking and mean length of speech yielded correlations significant at the .07 and .02 levels, respectively. A look at the direction of speech measures shows that two of the four measures met the usually accepted level of confidence while two failed to reach statistical significance. The data for the control interviewer is not presented in tabular form, but it should be noted that he correlated significantly with *E*2 on the frequency measure and with *E*1 and *E*2 on the duration and mean length of speech measures. In addition, although *E*1 and *E*2 correlated highly on the direction measures, they also tended to correlate highly with *Ec*.

Combined Results.

Table 4 presents the combined duration, frequency, and mean length of speech responses to *E*1 and *E*2 for all three experiments (N=16), all of which correlate significantly beyond the .0005 level. When the direction of response data was combined for Experiment II and Experiment III (N=11), it was found that responses to *E*1 and *E*2 were correlated beyond the .01 level on two measures and beyond the .02 level on the other two measures.

DISCUSSION

The data strongly suggest that *Es* can become near-equivalent stimuli in a group situation by following simple, content-free rules. Particularly striking was the consistency in the number of minutes each S spoke in the presence of *E*1 and *E*2, for in each of three separate experiments a perfect rank correlation was found. Although significant correlation coefficients were not found in some of the other measures in the individual experiments, it should be remembered that when one is working with very small *N*s, a single reversal in ranking can enor-

TABLE 4

MEANS, RANGES AND COEFFICIENTS OF CORRELATION FOR *E*1 AND *E*2. THE MEASURES REPRESENT A COMBINATION OF Ss FROM EXP. I, II, AND III. THE DATA REPRESENT MEAN RESPONSES TO *E*1 AND *E*2 PER 25 MIN. SESSION

Measure	*E*1	*E*2	*N*	Rho	*P*
Minutes of Speech			16	.93	.0005
Mean	3.10	3.29			
Range	.67-9.56	.50-7.83			
Freq. of Speech			16	.80	.0005
Mean	14.01	13.78			
Range	6.00-24.67	5.67-24.50			
Mean Length Speech			16	.93	.0005
Mean	.21	.25			
Range	.07-.48	.07-.47			
Freq. ea. S was addressed			11	.79	.01
Mean	12.54	12.07			
Range	9.25-20.31	3.5-22.5			
Freq. ea. S addressed *E*			11	.81	.01
Mean	8.90	8.00			
Range	4.25-15.30	1.25-16.50			
Freq. ea. S was addressed by Participants Other than *E*			11	.70	.02
Mean	5.10	6.59			
Range	1.30-12.00	.00-15.50			
Freq. ea. S Addressed Participants Other than *E*			11	.72	.02
Mean	5.22	6.75			
Range	2.00-13.75	1.50-12.75			

mously lower the coefficient of correlation. By combining Experiments I, II, and III, we were able to obtain the high correlations on all the measurements which we eventually hope to obtain from very small groups.

An unexpected finding in Experiment III was that a control interviewer, unfamiliar with the interview rules, elicited responses from the Ss somewhat comparable to those elicited by the standard interviewers. Perhaps this was partially due to the fact that the control interviewer provided much the same stimulus value as the other *E*s, both in appearance and in the nature of his verbalizations. The most parsimonious

explanation might be that the Ss would tend to make the same kind of content-free responses to any interviewer regardless of whether he followed interview rules, and in such an event standard rules would be unnecessary. The writers are more inclined to attribute the finding to the fact that three of the Ss in Experiment III were extremely delusional and attended only aperiodically to the *E*s as stimuli. Unless the *E* is a discriminated stimulus, no effect from following a procedure of any kind could be expected to emerge. The standardized interview as a method of obtaining baseline behavior may be contraindicated in such a population. Plans are now being made to replicate Experiment III on a less verbally tangential population.

It was noted that interaction in the standardized interview exceeded that usually experienced with regular therapy groups at this hospital. Perhaps the combination of regular periods of verbal activity and inactivity on the part of the therapist should be explored as a therapeutic tool in group psychotherapy.

In addition to trying to refine our present methodology, we propose to systematically investigate other content-free measures such as initiation, latency, rate, etc., and to examine the reliability of all these measures over a period of time. Our overall aim is to obtain a reliable baseline of content-free verbal behavior for each S as he functions in a small group situation, since assessing changes in individuals rather than groups is our goal.

SUMMARY

Two *E*s separately interviewed 5 chronic neuropsychiatric patients in Experiment I, 5 acute neuropsychiatric patients in Experiment II, and 6 additional chronic neuropsychiatric patients in Experiment III. Standardized group interview rules were employed by the *E*s, and the Ss' responses, in terms of content-free measures, were recorded. The combined experiments yield highly significant correlations on all measures between Ss' responses to *E*1 and *E*2. The experimental findings suggest that it will be possible to develop a list of highly reliable, content-free measures applicable to the group situation, which could be utilized as dependent variables in evaluating group psychotherapy.

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PERFORMANCE IN BRIGHTNESS DISCRIMINATION LEARNING AS A FUNCTION OF TIME OF FOOD DEPRIVATION (h)[1]

SEYMOUR H. BARON[2]
Tulane University

Although much theoretical effort has been devoted to the problem of discrimination learning, little has been done to relate it to motivational variables. Strictly quantified predictions are not possible at present because of the indeterminate values of many variables having presumed significance for discrimination learning. In this connection, it should be noted that many of Hull's parametric values (Hull, 1952) were assigned for purely expository purposes.

Many empirical attempts to relate time of deprivation to discrimination learning have yielded inconclusive results. A number of studies (Macduff, 1946; Ramond, 1954; Seward, 1948; Teel, 1952; Tolman & Gleitman, 1949) have indicated a facilitatory effect in discrimination learning with increasing hours of food deprivation, while other studies (Dinsmoor, 1952; Hillman, Hunter & Kimble, 1953; Kendler & Mencher, 1949; Meyer, 1951; Teel & Webb, 1951) have indicated no such relationship. Most of these studies utilized maze-type spatial discrimination situations in which the measure of acquisition has been defined as the number of trials necessary to reach some arbitrary criterion of learning.

Recent attempts have been made to relate discrimination learning to anxiety test scores, the assumption being that scores so obtained are indicators of generalized drive level (D). Spence and Farber (1954), using tones as the CSs, an air puff as the UCS, and the eyeblink as the CR, have found a trend toward facilitation of discrimination with increased D. Hilgard, Jones and Kaplan (1951), using essentially the same procedures, have obtained data indicating that increased D retards discrimination learning.

Following Hull (1943), Ramond (1954) has predicted choice behavior in a two bar Skinner box as a positive function of drive. Strictly speaking, Ramond was dealing with a response selection, rather than a discrimination situation. However, an almost identical formulation has been used (Spence & Beecroft, 1954; Spence & Farber, 1954) to predict the acquisition of discrimination in human Ss. In the present experiment, essentially the same predictive procedure is to be utilized

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[2] Now at Psychopharmacology Service Center, National Institute of Mental Health, Bethesda, Maryland.

with latency of response as the measure and discrimination between stimuli as the task of the subject. In the development of this notion, the following notation will be used:

$_{s}t_{R}+$ = latency of response (R_1) to the reinforced simulus (S_1)
$_{s}t_{R}-$ = latency of response (R_2) to the nonreinforced simulus (S_2)
$_{s}E_{R}+$ = reaction potential to S_1
$_{s}E_{R}-$ = reaction potential to S_2
H = habit strength to S_1
$\overline{H}$ = generalized habit strength to S_2 as a result of stimulus generalization.

Following a development analogous to Ramond's,

then $\quad {}_{s}t_{R}+ - {}_{s}t_{R}- = f({}_{s}E_{R}+ - {}_{s}E_{R}-).$

Since $\quad {}_{s}E_{R}+ = f(H \times D)$

and $\quad {}_{s}E_{R}- = f(\overline{H} \times D),$

then $\quad {}_{s}t_{R}+ - {}_{s}t_{R}- = f(HD - \overline{H}D)$

$= f[D(H - \overline{H})].$

From this development, it follows that as the value of D increases, the difference between $_{s}t_{R}+$ and $_{s}t_{R}-$ should also increase. Thus, performance in a discrimination situation is predicted to be a positive function of drive, i.e., discrimination should be facilitated with increasing hours of food deprivation (h).

METHOD

Subjects. Ninety Long-Evans male rats were used. They were approximately 6½ months old at the start of discrimination training. The animals were secured from the animal colony of the Psychology Department of Tulane University.

Housing and Experimental Room. The Ss were housed in the animal room of the Psychology Department, with squads of five animals (which were run at the same hour) assigned to each cage. Temperature fluctuations were not recorded. The experimental room was an air-conditioned, insulated cubicle approximately 7 x 8½ feet. The insulation also served to sound dampen the room. The temperature in the experimental room varied between 68 and 73 degrees F.

Apparatus. The apparatus replicated that used by Wilcoxon, Hays, and Hull (1950). It consisted of a starting and middle chamber (separated by a guillotine door, GD) which were identical in size, and a larger goal box (which was separated from the middle chamber by a one-way swinging door, SD). It was painted a neutral gray midway between the black and white discriminanda which were Munsell coated papers of 1.21% (black) and 78.66% (white) the reflectance of magnesium oxide. The discriminanda were presented on the middle chamber side of the SD, which led into the goal compartment. Latency ($_{s}t_{R}$) was

defined as the time elapsing between the raising of the GD 1 in., and the pushing outward toward the goal box of the SD ½ inch.

Feeding Cycle. All Ss were placed on the same feeding cycle which consisted of 48 hr. food deprivation followed by 24 hr. of *ad libitum* feeding. The animals were run at the appropriate time (3, 24, and 48 hr. after food removal) within the cycle. Thus, each feeding cycle consisted of three days. This cycle was utilized in an attempt to obtain proportionate weight losses and to equalize mortalities among the groups. The Rockland Rat Diet was used, and was supplemented on alternate cycles with a tablespoonful of wet mash composed of finely ground Rockland flour, water and cod-liver oil. Water was available in the living cages at all times.

Handling, Habituation to the Apparatus, and Preliminary Training. Each animal was handled for a period of three weeks, until cessation of squirming and squealing. Habituation (days 1-10) consisted in placing squads of five animals in the apparatus for 10 min. each day for 10 days. By the end of this period, the Ss were able to manipulate the SD.

The animals were placed on the feeding cycle during days 11-23 (four cycles) and were not handled during this period. Preliminary training occurred between days 24-26, from 12:00 noon to 6:00 P.M., each squad being run during its scheduled hour. By the end of this phase, the animals were able to perform all the necessary behaviors required in discrimination training, i.e., manipulating the SD and finding the reinforcement pellet. A gray Munsell paper, midway between the black and white discriminanda was inserted in the SD during this training, and the .94 mg. pellet was found on an identical gray paper in the goal compartment. A total of 16 pretraining trials were given, the last four trials being completely unassisted by *E*. All electrical contacts were connected during the pretraining in order to habituate the rats to their operating noises.

Discrimination Training. The running schedule was identical to that used during pretraining, and running commenced approximately 3 min. after the animals were brought into the experimental room. Each animal was given four reinforced and four nonreinforced trials per cycle with the trials being distributed by giving one trial to each animal, then a second trial to each, and so on. The time elapsing between each animal's trials was held to a minimum of 3½ minutes. The reinforcement order was NRNNRNRR, where R and N refer to reinforced and nonreinforced trials, respectively.

On each trial, S was removed from its carrying cage and placed in the starting compartment. The glass lid covering it was then closed. The GD was not raised until S stood oriented towards it. A minimum of 5 sec. elapsed before opening the GD, regardless of the animal's position relative to it. If the door pushing R was not followed by entrance

into the goal compartment, the animal was removed from the apparatus, returned to its carrying cage, and the latency of the door pushing R was recorded. This almost never occurred in the presence of S_1 and infrequently in the presence of S_2.

For the first 30 cycles, the maximum allowable latency was 300 sec., after which time the animal was removed from the apparatus and returned to its carrying cage. On cycle 31, the maximum allowable latency was reduced to 120 seconds.

On reinforced trials, a .94 mg. food pellet was placed on top of a piece of paper corresponding in brightness to the reinforced discriminandum. On nonreinforced trials a piece of paper corresponding in brightness to the nonreinforced discriminandum was placed in the same position in the goal compartment with no food on it. Each animal was detained in the goal compartment from 12-15 sec., on both reinforced and nonreinforced trials.

RESULTS

Due to the loss of animals during the course of training, latencies were not always recorded for the same number of Ss, either between groups or blocks of cycles. Table 1 indicates the number of animals that were run for each block of cycles, in each group.

TABLE 1

THE NUMBER OF Ss IN EACH GROUP ON EACH BLOCK OF CYCLES

h gp.	Cycles		
	2-13	14-25	26-37
3	27	27	27
24	27	24	24
48	23	23	22
Total	77	74	73

The latency data were prepared for analysis by using the following procedures: (a) the median score for the last three trials of the reinforced stimulus (S_1) and the median score for the last three trials of the nonreinforced stimulus (S_2), for each animal, in each group, and on each cycle, was obtained. The data of the first two trials (trial 1 was nonreinforced while trial 2 was reinforced) for the cycle were eliminated because of warm-up. (b) The difference between these medians ($S_2 - S_1$) was calculated for every animal; and (c) for each of the above measures a median was calculated for each block of 12 cycles (2-13, 14-25, 26-37). The data obtained from step (c) are presented in Table 2, and $S_2 - S_1$ is graphically illustrated in Fig. 1.

A logarithmic transformation was applied to the data for cycles 26-37 for $S_2 - S_1$. A subsequent Bartlett's test of this data, however, yielded

a highly significant chi-square value which precluded the use of the analysis of variance technique. Consequently, a nonparametric approach to the data analysis was used, the Kruskal-Wallis test (1952) being selected primarily because it allows the comparison of more than two groups. The data fulfilled the assumptions underlying this test.

Table 2 and Fig. 1 show that, with regard to the medians calculated for $S_2 - S_1$, the difference between S_2 and S_1 decreases as h increases. The Kruskal-Wallis test was applied to the rank scores of the data from which these medians were calculated in order to evaluate

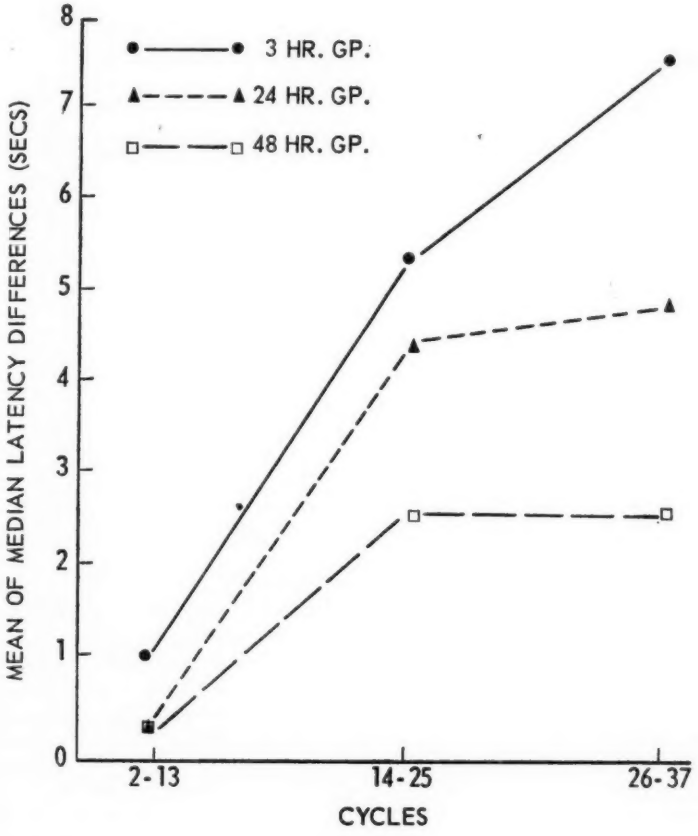

Fig. 1. Latency differences for the three deprivation groups.

TABLE 2

MEDIAN LATENCIES UNDER EACH DEPRIVATION CONDITION, IN EACH BLOCK OF CYCLES, AND FOR EACH VARIABLE

Variable	*h* gp.	Cycles		
		2-13	14-25	26-37
S_1	3	1.32	.92	.70
	24	.85	.88	.68
	48	.79	.76	.64
S_2	3	2.55	6.33	8.24
	24	1.09	5.43	5.59
	48	.90	3.48	3.32
S_2–S_1	3	1.00	5.38	7.47
	24	.29	4.52	4.86
	48	.27	2.67	2.62

the differences among these distributions. The results of the test in terms of the *H* statistic and associated probabilities are shown in Table 3.

TABLE 3

TABLE OF THE *H* STATISTIC FOR DIFFERENCES AMONG DEPRIVATION CONDITIONS

Cycles	Variables		
	S_1	S_2	S_2–S_1
2-13	7.845*	6.920*	2.369
14-25	2.573	11.829**	12.514**
26-37	1.541	16.421***	17.730***

*Significant at the .05 level.
**Significant at the .01 level.
***Significant at the .001 level.

In terms of the decreasing differences between latencies ($S_2 - S_1$) with increasing *h*, the data reveal that as hours of deprivation increase (within cycles 14-25 and 26-37), the differences in latencies between S_2 and S_1 decrease, i.e., an inverse relationship between performance in discrimination learning and hours of food deprivation is suggested. It is to be noted that for cycles 14-25 and 26-37 the null hypothesis cannot be rejected with respect to the S_1 variable. Since the null hypothesis *can* be rejected with respect to the S_2 variable, then any differences obtained in $S_2 - S_1$ must be due to differences with respect to S_2. This relationship does not hold for cycles 2-13. The implications of these results in terms of gradients of stimulus generalization and the gradient of incidental stimulation (S_3) will be treated in the Discussion section.

DISCUSSION

It was predicted that increasing h would facilitate discrimination learning. However, results contradictory to this prediction were obtained. One approach to an explanation of this data might be in terms of the relationship between h and the gradient of stimulus generalization. In this connection, the data obtained by Brown (1942) are revealing. In a straight runway situation, two groups of differentially deprived rats (1 hr. and 46 hr.) were trained to go to food which was associated with a screen of high intensity (5000 ft. candles) for one group, and a screen of low intensity (.02 ft. candles) for the other. One of the measures obtained was mean starting time (latency) following presentation of the test stimulus. The data demonstrate a much steeper gradient of generalization for the 1 hr. group than for the 46 hr. group. In fact, the gradient for the 46 hr. group approaches a horizontal straight line, the wide separation of the stimuli notwithstanding. "Thus one is led to the conclusion that a reduction in motivation tends to produce more decrement in the strength of generalized responses than in the strength of conditioned responses" (Brown, 1942, p. 224). This statement receives substantial verification in the present data. For S_1, a significant H value was obtained only in the first block of cycles, whereas this value declines with continued training. For S_2, on the other hand, significant H values were obtained for all three blocks of cycles, with the H value increasing in magnitude with continued training. Thus, decreasing slopes of the generalization gradient with increasing h may be a possible explanation for the data.

Another possible explanation may lie in the development of $S{-}{>}R$ connections between incidental stimuli (S_3) and reinforced responses (R_1). Hull (1952) has taken $S_3{-}{>}R_1$ into account in terms of its contribution to reaction potential in the discrimination situation. Sources of stimulation entering into the S_3 complex are lights and shadows, consistent odors, apparatus noises, etc. In addition, Hull has included ". . . the infinitely complex stimuli coming from the animal's own body" (Hull, 1952, p. 64) as part of the S_3 complex impinging upon the organism during reinforcement. An important part of this internal stimulation is comprised of the drive stimulus (S_D) ". . . which is a monotonic increasing function of this state" (Postulate V C, Hull, 1952, p. 7).

Since these stimuli are present during the evocation of responses followed by reinforcement (R_1), the condition $S_3{-}{>}R_1$ develops. Furthermore, since R_1 is evoked by S_1 and S_3, Hull assumes that their reaction potentials combine additively. Two implications follow from this analysis: (a) the slope of the generalization gradient if flattened and (b) the asymptote of the generalization gradient is raised (Theorems 11A and 11B, Hull, 1952, p. 66).

In the absence of relevant data, let us assume the equal impingment of S_3, other than that portion comprised of S_D, on all groups.

This implies the restriction of differential stimulation arising from S_3 to the S_D component. From Postulate V C (Hull, 1952, p. 7), the magnitude of S is related to *h*. Now, there is some justification for the assignment of some of the formal properties of a conditioned stimulus to the drive stimulus. If this is a reasonable assumption, then Rs in the presence of the S_D must be controlled by those variables governing the relationship of external CSs and Rs.

It has been shown that under certain experimental conditions, strength of conditioning, as inferred from measures of response strength, is related to the intensity of the CS (Bragiel & Perkins, 1954; Hull, 1951; Perkins, 1953). If this relationship holds for S_D and R as well as for CS and R, then it can be asserted that strength of conditioning will be heightened with increasing intensity of S_D. In terms related to the present experiment, this means that the $S_3 -> R_1$ connection is increased in strength as *h* increases, and is reflected in the apparently flattened and heightened gradients of generalization as a function of increasing *h*. This may be a useful explanatory principle for the retardation of discrimination with increasing *h*.

In order to account for the eventual appearance of the stimulus generalization gradient, Hull assumes that the $S_3 -> R_1$ connection is eventually neutralized through the process of experimental extinction (Theorem 12, Hull, 1952, p. 68). Theorem 12 asserts that with continued training, the control of the incidental stimulation becomes relatively unimportant in the determination of overt behavior.

The complete eventual neutralization of $S_3 -> R_1$ under all experimental circumstances is doubtful. Since the data indicate increasing retardation of discrimination learning as a function of *h*, it may be possible to assert that $S_3 -> R_1$ approaches an asymptotic value as some function of *h*, which is greater than zero. This leads to the suggestion that the attainment of perfect discrimination under all values of *h* may be impossible.

On the other hand, if S_D is *not* an increasing monotonic function of *h*, there may be some optimal deprivation level leading to eventual perfect discrimination. That is, up to some point on the *h* continuum, S_D may increase and then decrease. To test this experimentally would involve protracted discrimination training under a number of values of *h*.

It becomes evident that these data have implications for the H x D formulation in a theoretical approach to discrimination learning. The assertion has been made that increasing D facilitates discrimination learning, since $H - \overline{H}$ presumably remains constant for all groups. The data indicate the existence of other possibilities, however. It may be that the slopes and asymptotes of the gradients of stimulus generalization change with different values of *h*, or the S_D component of

S_3 may be a more potent determinative factor than the multiplicative relationship of H and D in discrimination learning. In fact, the S_D component of S_3 may completely override, to the point of obscuring, the multiplicative effects of H and D.

SUMMARY

On the basis of previous theoretical and experimental work, a formulation was developed which permitted the prediction, in a discrimination learning situation, of facilitation of discrimination learning as a function of hours of food deprivation (h). Discrimination learning was measured in terms of differences in latency of response (st_R) to the reinforced (S_1) and nonreinforced (S_2) discriminanda.

The results, which were analyzed by means of a non-parametric test, indicated that increased h retarded rather than facilitated learning, thus forcing a conceptual reorganization with respect to the relationship between the H x D formulation and discrimination learning. This new conceptualization was organized in terms of gradients of stimulus generalization, the gradient of incidental stimulation and drive stimulus (S_D) intensity.

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GEORG VON BÉKÉSY, *Experiments in hearing,* (Translated and edited by E. G. Wever), New York: McGraw-Hill, 1960, Pp. 745. $25.00.

Dr. von Békésy is a master experimenter. He has performed delicate surgery, used ingenious optical systems, cleverly automated behavioral experiments, artfully applied his knowledge of electronics, vibration, and acoustics, and skillfully used physical, biological, and mathematical models for purposes of demonstration and investigation. For psychological experiments his choice of the subject's response is adaptive. When a simple motor response is required, it is used; when an introspective report in the form of speech or a drawing is useful, it is obtained. Not bound by a restrictive philosophy of psychological science, he has selected methods appropriate to the problems; he has not chosen problems because they were appropriate to a few rigidly preconceived methods. He shows us that the solution to an experimental problem can be reached by coupling the virtues of knowledge and tenacity with flexibility in approach. Beyond the solution of experimental problems, his accomplishments beautifully demonstrate the joint use of bio-physical, anatomical, and psychological data to aid in the understanding of man's interaction with the environment.

The principal publications of Georg von Békésy are collected in the four major sections of *Experiments in Hearing.*

Of general interest to psychologists is the introductory section. Dr. von Békésy's interesting views on scientific method are stated with art and humor in the first chapter. In the remaining three chapters of this section, his experimental techniques and innovations are described.

Collected in another section are Dr. von Békésy's papers concerning the perception of sound. Included among the many topics are pitch and loudness discrimination, perstimulatory and poststimulatory fatigue, investigations at the frequency and intensity limits of the ear, and the localization of sound in space. Although almost all of the areas of audition are treated, this is not a text-book. But, after covering an area in secondary sources, the student should turn to *Experiments in Hearing* for more detailed descriptions of von Békésy's experiments; in addition, he will find many experiments that have been overlooked by the authors of secondary sources and later journal articles.

Dr. von Békésy's famous and most important papers on the biophysics of the ear are presented in the remainder of the book. Also included are the recent and especially interesting papers which describe the use of the skin as a model for the basilar membrane. While there are difficulties with this analogy, this is material with intrinsic value from an exciting frontier of knowledge.

Dr. E. G. Wever has added to his own list of contributions by acting

as editor and translator. In addition to translating many papers from German to English, he has carefully arranged the materials by subject matter and insured smooth transitions. This volume can be read almost as easily as one written as a single manuscript. One does wish that Dr. Wever had used the standard terminology recommended by the Acoustical Society of America and that better summary statements had been provided by Dr. von Békésy.

This book is too expensive and specialized to be recommended for purchase by all psychologists. No departmental library should be without it, and every experimental psychologist, regardless of his area of specialization, will profit from several evenings with it.

Central Institute for the Deaf JAMES D. MILLER

Kronhausen, Eberhard, and Kronhausen, Phyllis, *Pornography and the law. The psychology of erotic realism and pornography.* New York: Ballantine, 1959. Pp. xvi & 317.

Contemporary society is becoming increasingly dependent upon professional advice in the conduct of everyday affairs. The Kronhausens' book not only manifests this trend, but also indicates the necessity for serious consideration of the widening area of professional involvement. The specific point which aroused this reviewer's concern was the Kronhausens' Freudian assumption that behavior is a response to inner impulses rather than to external stimuli. This assumption leads naturally enough to the conclusion that literary censorship to protect society is unnecessary and undesirable since sexual literature allows catharsis to occur. However, Wertham in his *Seduction of the Innocent* makes the opposite assumption and argues that censorship is necessary in order to protect society! Resolution of the theoretical conflict concerning the casual nature of impulses and stimulus objects might be accelerated if scientifically competent psychologists would devote a portion of their research energies to the role of literary stimuli in the production of patterns of sexual behavior. At the least those psychologists who testify in censorship cases would have objective evidence to refer to.

The Kronhausens skillfully summarize the present legal status of pornographic literature and present an impressive sampling of the sexual literature produced by our culture. The distinction they draw between "hard core pornography" and "erotic realism" (both copiously illustrated) appears valid and may prove to be of considerable utility in the future.

Denison University PAUL T. MOUNTJOY

The Psychological Record, 1960, 10, 319-326.

KOCH, S. (Ed.) *Psychology: A Study of a Science*: Study I. Vol. 1. Sensory, Perceptual and Physiological Formulations, Pp. ix + 710. $9.75. Vol. 2. General Systematic Formulations. Learning and Special Processes, Pp. 706, $10.00. Vol. 3. Formulations of the Person and Social Context, Pp. vii + 837, $12.50.

These are the first of seven volumes meant to serve as a picture of psychology, sponsored by the American Psychological Association and financed by the National Science Foundation.

One is tempted to make some comparisons between this effort and the concurrent one made by the American Physiological Society, which is producing a multi-volume Handbook after the old German tradition. Whereas the project of the physiologists is the larger, it is not embellished by explanatory and propaganda-like chapters. The Editor's explanatory chapters in the first volume in the present project are grandiose, fatuous, and pompous, and fail to give the reviewers a clear picture of what was attempted in the project. They did not succeed in stating the guiding principles for inclusion of material. Hence, many readers may find, to their surprise, some things not expected, and be disappointed in not finding certain others. Be this as it may, many of the individual chapters provide most excellent systematic formulations of position, surpassing everything else in the literature in compactness, completeness and adequacy of expression. One has the feeling that most of the authors put themselves completely into their tasks. Perhaps, behind the scenes, the editor and the committee did a unique job of eliciting this sort of effort from the individual contributors, for it can be said the character of most of the chapters is anything but perfunctory.

VOLUME ONE

In this volume, eleven chapters deal with the following topics respectively: auditory theory (Licklider), color theory (Graham), quantum theory of light (Pirenne and Marriott), stereoscopic vision (Ogle), the Luneberg theory of space perception (Blank), the systematic theory of Köhler (Prentice), perception as a function of stimulation (Gibson), Brunswik's probabilistic functionalism (Postman and Tolman), adaptation level theory (Helson), neurophysiological theory (Hebb), physiological theory of drive (Morgan).

An appendix contains a copy of the document sent to the contributors in regard to the discussion themes to follow. The editor and the committee are to be commended for actually attempting to guide the authors in the conceptualization of the tasks that were asked of them. While some of the authors frankly stated they were unable to follow the prescriptions in a formal way, the document probably served its purpose, nevertheless.

There is no space to comment in a significant and specific way on

each of the chapters. All of them in this volume were excellent from the standpoint of author competence and scholarliness. We were not able to tell what the rationale of including quantum theory discussion for psychologists was, i.e., in the light of the omission of chapters on the lower senses. Since this project deals heavily in theory, the lack of an ideal body of experimental facts in an important sensory area should not rule out some discussion of it. Surely, for example, the sensory areas of kinesthesis, the tactual and vestibular senses and pain did not merit being ignored.

The character of discussion of the Luneberg Theory of space perception was not so as to make it useful to most of the psychologists, who, otherwise, might be able to comprehend and use it. One wonders how many more presentations of the Luneberg theory he is going to encounter, which will do small service in making clear the general problem and the contribution made by mathematics to its study and solution. It is time that a psychologist's version of the Luneberg theory is stated.

It is unfortunate that in the portrayal of Köhler's systematic position, Köhler himself was not the author. This is not disparagement of Prentice, but it seems that if the matter is important enough to find a place in the project, Köhler should have been the man to do the work.

The reviewers question the significance of Brunswik's probabilistic functionalist concept, except for the fact that many students may have been taught it. We say this in light of the exclusions already mentioned and the fact that transactionalism was not given a similar hearing.

It is in *Gibson's* chapter that he is at his best and for the first time seems to make clear what he means when he asserts the full determination of response by the stimulus situation.

The unique facility of *Licklider* shows up in his chapter on auditory theory and all psychologists would do well to read it even if they are not in the least interested in the sensory processes.

Graham's huge chapter on color theory is a most able presentation of the material and problems involved in this area. In fact it is probably the best single place in the literature to consult for this purpose.

Helson's lucid chapter is the best statement anywhere of adaption level theory and what it pertains to.

Hebb's very short chapter on neurophysiological theory has to do with matters that are possibly much more diffuse and nebulous than those dealt with by the other authors. The chapter does not show the same concentration on the subject that is characteristic of the other chapters. The net result is that the chapter leaves one unsatisfied.

VOLUME TWO

This volume titled "General Systematic Formulations, Learning, and Special Processes" contains chapters contributed by twelve authors, but is poorly titled. Seven of the chapters discuss behavior from the position of more or less empirically based "learning theory," three apply formal interpretations of behavior extraneous to psychology, one is an expansion of Lewinian theory, and one a chapter dealing with comparative behavior.

Clearly empirical standpoints are represented by Hinde, "Some recent trends in Ethology;" Irion, "Rote learning;" Harlow, "Learning set and error factor theory;" and Skinner, "A case history in scientific method."

The *Hinde* contribution consists of a commendable, complete enumeration and definition of the broadest sort of conceptual problems within the field of comparative behavior, a summary of attempted solutions made chiefly by European workers, and a distillation of principles he can organize from the reported work. There is no attempt to present a formal systematization of the field. Unfortunately physiological, neurophysiological, endocrinological, pharmacological, and genetic perspectives are all but unmentioned. The chapter is one of the shortest in the book even though the systematic considerations he outlines have by far the widest scope.

Irion inaugurates the chapter dealing with rote learning by discussing possible bases upon which scientific activity might rest. Interestingly he points out that once a start has been made within a problem area, organization may be effected simply by repeated and extended use of established concepts. In rote learning the organizing concepts used are apparently those that have been empirically discovered to facilitate communication. Enlarging upon this, it is pointed out how both the concepts and hence the directions of work have depended upon only a few workers and the British associationistic tradition. Several pages are devoted to a discussion of methodological factors that make the conventional variables difficult to use as names for classes of events. Irion suggests that rigid standardization of procedural factors could endow conventional rote learning variables with a specificity that would encourage general attempts at theorizing. At present only limited generalizations exist. Surprisingly Clark Hull's *Mathematico-deductive Theory of Rote Learning* is not considered in this regard. The last major section in the chapter attempts to show how in the case of an isolated problem, i.e., distribution of practice, increased precision in definition of experimental variables might promote more rigorous prediction, and how this in turn might lead to a theoretical consolidation with possibilities for fruitful hypotheses.

Harlow gives the barest account of the presumptive features entering into his theoretical excursions. Orienting factors are neglected. By implication, the formative factors entering into theory consist entirely of

inferences made from observations and data. Departing from this rather narrow basis Harlow makes a coherent statement regarding various of the technical aspects relevant to the understanding of learning set phenomena and then at some length develops some theoretical implications of the data emanating from the Wisconsin Primate, and affiliated Laboratories.

Skinner's chapter may already be familiar to many readers since it was published separately as an article in *The American Psychologist* several years ago. Skinner, of course, denies theory the central position in research and science that contemporary epistemology accords it. For this reason he centers on the background influences, orienting presuppositions, and working methods underlying his own work by empirical analysis. On the basis of his own case history he suggests several principles possibly playing a role in creative scientific endeavor. The consequences of science for technology and technology for science are given routine exposition. The roles of statistical methodologies and inference are critically scrutinized. It is the shortest contribution of the volume and on a very general level.

It is easy to both empathize with *Tolman* and to sympathize with his theoretical position. He so frankly acknowledges and expounds systematic biases having as their underpinnings "intuition, commonsense, phenomenology, and very general notions about neurophysiology." Nevertheless the chapter really constitutes a rewarming of a grand scheme that Tolman himself acknowledges in the first paragraph to be "pretty much passé." As is customary with Tolman he makes a rather abstract analysis of five experimental set-ups selected to exemplify typical theoretical analysis. Operational features at every level of analysis are considered however. There is also a fragmentary classification of "laws of learning" into his system and a hurried reference to motivation. The most extended specific discussion concerns performance, a type of response in which "manipulatory organism-environmental rearrangements" occur. One often wonders when reading Tolman what curious factors directed him to apply intuitions, common-sense, phenomenology, etc., he has about humans to rats in human artificial environs. The result has been a delicate but too frail structure and we are not much the wiser after reading the chapter concerning factors determining its existence.

Guthrie attempts a frank and penetrating analysis into his own theoretical behavior and personal predilections. This is followed by a discussion of science, characteristics and scope of theory, etc. The remainder of the chapter is a critical consideration of contiguity as a learning principle. Guthrie is severely critical, pointing out that he is not convinced that in terms of association by contiguity, learning theory has advanced through the multitude of laboratory studies since Aristotle. He admits awareness of his "failure to lay down specifications for the data of learning which are sufficiently precise to meet the requirements of laboratory experiment." Guthrie combines acumen with

considerable epistemological sophistication. In the reviewers' opinion, this is one of the finest chapters in the volume.

Miller approaches systematic clarification somewhat differently than the preceding learning theorists. The paper is of two parts. Part I contains a perfunctory list of acknowledgements, but it seems that his exposition of approach-avoidance theory is the real background for what he has to say. The theory itself is tied together at a very abstract if not purely verbal level. This occurs, of course, because Miller chooses to use laboratory findings largely as illustrations of broad common-sense variables. These variables are generalized to other problem areas, chiefly clinical, child, and social psychology. In part II, Miller describes the position of such a theorist on a host of issues and also outlines the direction future approach-avoidance research and theory might take. The most extended arguments concern the nature of the S-R relationship, the principle of reinforcement, and intervening variables. The discussion of scaling is commendable. Apparently he is aware that when one chooses to express behavior by numbers, one must take great care not to use a number system permitting deductions about behavior incompatible with observation.

Logan's chapter deals with latter-day Hullian theory. Anyone acquainted with academic psychology a decade ago will perhaps find it difficult to associate the theoretical modesty and restraint found here with the Hull-Spence approach to learning theory. At this historical juncture, however, the mathematico-deductive approach is interpreted as a commitment to a method of theorizing rather than a commitment to any particular set of theoretical ideas. This major qualification allows an easygoing comparative description of Hullian and Spencian theories and appraisal of their empirical status. Several new items are given theoretical attention from the S-R standpoint. That is, problems of "correlated reinforcement" are introduced, analyzed (using a macromolar-micromolar distinction), and resolved in "the equilibrium solution."

Estes deals first with epistemological orientation. He stands for physicalistic, operational, empirical viewpoints, though his discussion seems rather superficial and provides somewhat idiosyncratic definitions of broad epistemological concepts. He then deals with the conceptual process underlying mathematical development and empirical generalization of his model. This is so skillfully done that readers with only the most general formal preparation will be able to follow. Part three is a formalization of the theory, a consideration of its empirical status, and an outline of the implicit methodology. The relation of statistical learning theory to other mathematical and verbal theories of learning is also briefly considered. Estes' chapter is the hardest to assess, perhaps because the topics given lengthy discussion are narrow if not esoteric. Certain successive changes may be detected within Estes' positions and program. First, the theory as a formal device seems to be growing much more rapidly than the body of data supporting it would seem to permit. In too

many instances, theoretical extensions can only be characterized as reckless and overly ambitious. Second, the proliferation by expediency of formal concepts which Estes himself so severely criticized in other learning theories a few years ago, seems to be appearing here all over again. Third, the mode of presentation suggests less the program, reflections and temperament of a scientist than that of a prophet. The following quotation reflects the character of the article.

> It would be convenient if we could settle each problem before moving on to the next. However, nature is not always so obliging as to permit this, and we cannot allow our general theoretical advance to be held up indefinitely by a few isolated pillboxes. While some of our forces remain pinned down by refractory problems associated with nonreinforcement and parameter invariance, we continue to make progress on other fronts.

Cartwright summarizes Lewinian theory, incorporating into it extensions made by others since Lewin's death. The chapter begins with a brief summary of formative ideas and events affecting Lewin's work. The direction researches have taken subsequent to Lewin's death is noted. The formal concepts of the system are reviewed and almost all are given satisfactory elucidation. Exceptions involve what Cartwright calls the "critical terms" of the theory. "Persons," "environment," and "behavior" are left as Lewin left them, obese, vague, formless, and shifting. One of the less routine sections deals with the possibility of translating topological concepts into mathematics. Apparently the major concepts of hodological space are not only expressible as linear graphs but these graphs in some cases make conceptualization more direct and satisfactory.

Frick classifies information theory as formal and normative. That is, he holds that information theory is (a) directly derived as an extension of probability theory, (b) a purely symbolic system and not presumed to model any specific behavior, and (c) not concerned in any way with the character of the concrete processes leading to the events under study. After making this clear, the theory itself is outlined, and its function as a conceptual framework for behavior analysis is dealt with. In its sophistication and exactitude, this is one of the best chapters in Volume II.

Ellson's contribution consists mainly in a description of Linear Frequency Theory. Once again one finds a purely formal and normative theory applied to psychology. The chief concepts of Linear Frequency Theory and information theory are compared. The psychological interpretations pertain to restricted motor behaviors. Serious difficulties in interpretations and the fact that the theory is in its elementary stage as a behavior model are acknowledged.

VOLUME THREE

This is a volume having to do with "formulation of the person and the social context." It is not the place here to attempt extended evaluations of the systems of the contributors to this volume. None of them is, of course, without merit. The editor has in this volume as well as in the first two, succeeded in getting the authors to divulge the kinds of thinking they have been doing, and these show their concerns and how their psychological systems have been developed. Nowhere except in pure autobiography will one be taken into the inside as much as in most of these chapters. It gives the reader a kind of comfort to feel that dodging and vagueness are at the minimum, and to use the vulgar expression, the authors "come clean."

Murray gives the impression of being a good artist rather than a theoretical and/or empirical scientist.

Rapaport, in writing on the structure of analytical theory, lays down seven rules or considerations involved in such a task as his. These are quite commendable. Rapaport states that psychoanalytic theory is based upon 10 propositions, including the following: (a) behavior is integrated and indivisible; (b) no behavior stands in isolation; (c) all behavior is part of a genetic series; (d) the crucial determinants of behavior are unconscious; (e) all behavior is ultimately drive determined; all behavior disposes of and is regulated by psychological energy (whatever that is!); (f) all behavior has structural determinants; (g) all behavior is determined by reality; (h) all behavior is socially determined. Whether one can agree with or have respect for what analytic theory turns out to be, one can say that Rapaport has done a good job in stating its essentials. His bibliography contains 330 items most of which deal with collateral though pertinent considerations in the writings of non-Freudians.

The third chapter is by *Rogers* who describes his "Theory of therapy, personality, and interpersonal relationships as developed in the client-centered framework." Here again we can only say that in the chapter the reader is given just about the most intimate and revealing statement of this theory to be found anywhere.

In the next chapter *Cattell* attempts to show what "multivariant quantitative" research is and thus to try to assure the reader that by the mathematical procedures used, one can arrive at natural entities that one must know about if he is to understand personality. Of this, he did not convince the reviewers.

The fifth author is *Kallman* who discusses the psychogenetic studies of twins. Although the chapter is an excellent one on the subject, it is not evident that the topic is coordinate with the preceding ones, i.e., has to do with an overall description of personality.

Although we have no criticism of the chapter written by *Asch*, it is not at all apparent why it is a part of the present volume for it is not

primarily a theory of personality, although it is labeled a "perspective."

Newcomb, in the next chapter, presents a system in such a form as to make it both understandable and usable (testable, etc.).

Katz and Stotland deal exclusively with attitude structure and change. It is questionable whether giving a picture of attitudes is a full theory of personality. Hence, we have again in the volume a well-structured discussion of a single dimension or aspect of human behavior rather than an attempt to state an overall theory of personality.

Lazarsfeld's chapter on "latent structure analysis" deals with such matters as traits, and dispositions, using the procedure of item analysis. The reader is given a good statement of what this approach to the study of behavior is like, and thus again we find this chapter one of the more adequate discussions of its kind.

In the chapter by *Thelen* on "work-emotionability theory of the group as organism," we are not sure whether we are dealing with psychology or sociology, remembering that psychology is single-organism-centered, while sociology takes a group of organisms as its unit. This does not mean that Thelen's chapter is less than very good, it only means that it would seem that it is sociology and doesn't belong in the volume.

The chapter written by *Parsons*, the sociologist, on "psychological theory in terms of theory of action," is a chapter that goes beyond psychology in the development of what is pretended to be psychology. In the sense that it attempts to show fundamental similarities between psychological, social and cultural systems (all being action systems) it is a valuable contribution. To the extent that it may tend to obliterate the principal distinction between psychology and sociology it has its drawbacks. It so happens that in the thinking of many psychologists the distinction between the single-individual as the constant reference and the group as the unit and point of reference, has too little significance or the wrong interpretation already. It seems that the "blurring" effect is quite prevalent from place to place throughout the Parsons chapter.

To the foregoing chapters, Koch, the editor, has appended an epilogue which refers to not only the material in this volume but the contents of volumes I and II. In this, Koch makes a very competent, insightful, well-put analysis of what has been transpiring in psychology in recent years, and it is well worth studying.

The final chapter (short) written by Conrad *Mueller* has to do with the "significance of sensory psychology for certain methodological problems." It is insightful and competent, and serves well to alert psychologist to certain problems overlooked by the other contributors in the third volume.

Michigan State University S. HOWARD BARTLEY
THOMAS M. NELSON

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